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Philosophy of Science

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The Theory of Emergence

BY

REUBEN ABLOWITZ

THE problem of the reality and nature of novelty in the universe has long engaged the attention of philosophers. "There is nothing new under the sun" is one ancient weighty utterance. On the other hand, "you cannot step twice into the same river", said Heraclitus, for in the interval between your first and second steps, the river has changed and you have changed. One recent attempt to analyze this problem and other affiliated problems, is the theory of emergence.

The theory caused a minor philosophic furor between 1926 and 1930, and, while no longer quite such a cause célèbre, it is still basic in many contemporary philosophies, including especially dialectical materialism. The term "emergent" was first used by G. H. Lewes (1875) in his "Problems of Life and Mind", but the concept was adumbrated by the "heteropathic laws of causation" of John Stuart Mill's "Logic" (1843). Indeed, analogous notions have been found in the writings of G. E. Stahl, J. C. Reil, and C. A. Rudolphi, in the eighteenth and early nineteenth centuries, and the theory has been approached in various ways by Claude Bernard, Herbert Spencer, and August Comte.

The theory itself, however, is due primarily to the writings of Samuel Alexander ("Space, Time and Deity") and C. Lloyd Morgan ("Emergent Evolution", "The Emergence of Novelty", etc.). The latter is a biologist who received his early inspiration

from Thomas Henry Huxley; he has been the chief protagonist of the theory. Other writers who in one form or another have espoused the concept are C. D. Broad (who used the term "emergent vitalism"), Roy Wood Sellars ("evolutionary naturalism"), E. G. Spaulding ("creative synthesis"), and Wilhelm Wundt ("creative resultants").

The theory declares, in brief, that the whole is more than the sum of its parts ("the appealing paradox"). It declares that there are levels of existence, and that there is a "tendency of units of one kind in combination, to constitute units of a new kind, with more complex constitution and new qualities due to the new togetherness of the parts" (Sellars). To illustrate with two simple examples:

If I play two notes together on the piano, there is an aspect or quality of this sound which is not the property of either of the notes taken separately. The chord has the characteristic of "chordiness"; the harmonious combination of sounds has a new attribute which no one of its individual components had, but which is due solely to their togetherness.

Similarly, if I place two atoms of hydrogen and one atom of oxygen in chemical juxtaposition, I get one molecule of water. Now both hydrogen and oxygen are gases, and water is a liquid; and the wetness of water is a characteristic that could not possibly have been deduced from the nature of its components; it is a *new* characteristic that is attributable only to the structural organization of the molecular level of existence.

The application of this to the physical world results in the hierarchy of atom—molecule—crystal. Just as the combination of atoms into a molecule reveals new and unpredictable qualities, just so does the structural organization of molecules into a crystal or colloid reveal characteristics which are non-significant or impossible at the molecular level, but attributable to the crystalline or colloidal level.

The application of this to the biological world indicates even more pregnant analysis, in the hierarchy of inorganic matter—life—mind. How the unique phenomenon of life can be deduced from inert matter, or how mental processes and reflective thought

can be deduced from a speck of protoplasm, are equally inconceivable: these are distinct levels of existence.

To this new quality of existence which results from the structural relation of its component parts, the name "emergent" has been applied. Chordiness, wetness, reflective thought—these are different kinds of emergents. As W. P. Montague has phrased it, "throughout the entire hierarchy of successively larger wholes, from the lowest sub-electronic elements up to the colloidal and crystalline masses that are visible to our eyes, we can expect to find (as in large and promising measure we have already found) a series of structures each of which will possess a nature different from the parts which compose it, but explainable in terms of the relations between those parts."

To this emergent, which is essentially *novel*, there is contrasted the "resultant", which is a property of the combination that *can* be foretold exhaustively from the individual elements. Thus in the example of the chord, I can tell precisely what the volume or loudness of the new sound will be if I know how loud each note is. Similarly, if I use bricks to build a house, the weight of the house is a *resultant* of the weight of the individual bricks, but the peculiar characteristic of being a new entity called a "house", is an *emergent;* you could not possibly tell from looking at a single brick what manner of object a house would be, unless you considered the structural relation the bricks were to assume. Resultant properties are additive; emergent properties are non-additive. Resultants may be predicted or deduced; genuine emergents may not. (I disregard here a distinction which some writers draw between apparent and genuine emergents, as tending only to confuse the issue.)

Thus we have a philosophic explanation of the process of evolution as well as of the nature of novelty. ". . . there are different natural systems to be reckoned with—mind-life-matter systems; life-matter systems; and matter-systems. At the top level there are modes of effective relatedness which are not present at the mid-level; at the mid-level there are modes of relatedness which are not present at the bottom level." (Morgan). Evolution is a process of continual emergence of successively higher levels of

N Y P L

existence, and each of these levels manifests something new in the universe. Novelties are constantly emerging. "No realm of fact, whether subsistent or existent, is exempt from this principle of creative synthesis, in accordance with which one or more specific organising factors so relate parts that there are new qualities in the resulting whole, and whole and part belong to specifically different modes of discourse." (Spaulding). And just what is it that is *new* at every evolutionary advance? It is some new *mode of relatedness*.

Thus the process of evolution is essentially jumpy or saltatory, rather than gradual; new kinds of structural relatedness emerge at specific temporal instants. The analogy is obvious to certain scientific laws in other fields: Mendeleieff's Periodic Law in chemistry, that in listing the chemical elements in order of atomic weight, new properties appear at specific points; the quantum theory of Planck in modern physics, that radiations from a body are in discrete amounts; the mutation theory of deVries in biology, that organic evolution is by sudden changes or "leaps" rather than by slow and successive degrees.

The theory of emergence thus accounts for the transformation of quantity into quality; it also accounts for the nature of those philosophic orphans, the so-called "secondary qualities", color-vision, for example. When I look at a red flag, is the redness in the flag, or in my head? In my head, says one school; in the flag, says another; in neither alone, say the emergent evolutionists. No one can tell from a complete knowledge of the number of vibrations required to produce red on the spectrum, and from a complete knowledge of the structure of the human retina and nervous system, what the color red will be like. The color is an emergent from the organisational relatedness between the two elements. Thus is answered the problem, what is the locus of the secondary qualities?

It is on the issue of unpredictability that the theory of emergence runs into opposition. That the antecedent phases of an emergent evolution do not permit of the prediction of the subsequent phases, is axiomatic. Novelty is inherently incapable of being foretold. A living creature without mental processes can

have no conception of the nature of reflective thought. The notion of predictability is of course temporal; that is, an eclipse is not an emergent, because, inter alia, its occurrence can be exhaustively and accurately predicted. The system of the universe predicated by the emergent evolutionists is constantly growing, changing, expanding, building up; it is part of the nature of the system that the precise character of the emerging novelty cannot be prophecied.

Is emergence for that reason, indeterministic? Not at all; either determinism or indeterminism is consistent with the assertions made. Morgan was himself a determinist; he believed that every event or emergence has a cause; novelty is itself "caught up in the web of causal nexus". Every emergent may be scientifically interpreted and apprehended, but only after it has occurred. Novelty is unpredictable but intelligible.

What is it, then, that *makes* emergents emerge? "What directs the course of events? . . . Some may say we know not and cannot know. Others may ask what need there is for a directive Source of emergence. Why should it not proceed without one? Yet others may urge that it is idle to put into an active Source just what is said to come out of it." (Morgan) If there is a natural plan of emergence, then every effect is strictly determined by the nature of that plan, and it is only our ignorance of it that makes the emergence of novelty such a mystery to us. (The theory does not allow, however, the influx into nature of an *alien* First Cause, whether called God or not.) Morgan himself asserted such a plan. (God is the causal efficient at every stage of progressive advance.) Alexander, however, postulated a great striving of all nature up to God, a cosmic process which he called "Nisus". God, in this analysis, would be the level just above that of human reflective thought. The analogy is obvious to the "esprit" of Bergson (rather than to his "elan vital"). J. C. Smuts, to whom is due the concept "holism", regards matter, life, mind, and personality, as successive steps in the progressive evolution of a single factor, the organic synthesising activity in the universe, a force the lowest manifestations of which are the chemical affinities.

Whether emergence is at variance with the time-honored belief

in the Uniformity of Nature, is another debatable point. Lovejoy defines emergence, or to use T. H. Huxley's more scientific terminology, epigenesis, as "any augmentative or transmutative event, any process in which there appear effects that . . . fail to conform to the maxim that 'there cannot be in the consequent anything more than, or different in nature from, that which was in the antecedent' ". He thus roundly denounces the ancient adage, "Ex nihilo nihil fit", which he terms "an arbitrary pseudo-axiom". There is no valid a priori argument against the possibility of absolute or general emergence, and every instance of specific emergence, no matter how slight, implies it. There is some substantive increment in the universe, and to this extent, the uniformity of nature goes by the board; it applies of course, up until the specific instance of emergence (which is as much as to say that it is a law except when it is broken). If, however, the Uniformity of Nature be conceived as synonymous with the existence of invariant causal laws to which all phenomena are subject, there is no antagonism between the theory of emergence and the Uniformity of Nature, for there are invariant laws of emergence. It is possible to make statements about the manner in which a molecule of water will "emerge" from the combination of two atoms of hydrogen and one of oxygen, and every observable instance of this emergence will conform to this statement. It is thus only in the narrower sense of "ex nihilo nihil fit" that the theory of emergence runs afoul of the beatified theory of the Uniformity of Nature; emergence is compatible with a thoroughgoing determinism, and yet explains and permits the introduction into the universe of new elements, elements which could have had before their emergence no existence whatever. The metaphysical question, How open is the future? may thus be answered, Wide open. The universe is constantly being enriched and diversified by the new organization of its elements, and the life ahead of us may be infinitely more profound than the life behind us.

The theory of emergence has proved an inspiration in a variety of fields far removed from that of biological evolution, where it originated. O. L. Reiser has drawn an analogy to certain problems in statistics: "If order can be produced from the 'chaos' of

random distributions through an averaging process, perhaps the 'emergents' . . . of evolutionary philosophy are but the 'central tendencies' of statistical distributions". A religious writer named Wm. C. Woods has found in the incarnation of Christ, an instance of the emergence of the human level into the divine.

It is on this question of levels that the emergent evolutionists find themselves in the greatest disagreement. H. C. Brown defines a metaphysical level as "a group of entities that are exemplars of the same set of specific laws", although certain general laws, e.g. gravitation, may apply to any or all levels. The difficulties with this "definition" are of course manifold, but it is an ex post facto attempt to define, as if it were a brand new term, one that has already had a long history. Perhaps the great variety of systems of levels is partially due to this ambiguity. At any rate, Morgan distinguishes matter, life and mind; Alexander sets up seven: space-time or motion, the primary qualities or categories (inherent in things), matter, the secondary qualities (conferred upon objects by our minds), life, mind, and Deity. Woods distinguishes the living from the non-living, the sentient from the non-sentient, the reasoning from the non-reasoning, and the social from the solitary. Morgan also indicates as separate subsidiary levels, sensory presentation, naïve perception, and contemplative thought; and he finds three levels in the emergent quality of memory: mere retention and registration, revival without the sense of psychical again-ness, and revival with this sense of psychical again-ness. This may seem to be drawing the lines too fine, but G. P. Conger declares there are no less than twenty-five levels: the sub-electronic energies, electrons, atoms, molecules, astronomical masses, solar systems, star clusters, galaxies, and astronomical universes (all this in cosmogony); in biology there are five, ranging from organic compounds to plant-animal groups; in sociology there are three, from families up to the "Great Society"; and in psychology, ethics, and aesthetics there are eight, ranging from specialized cells up to sentiments, values and personalities.

Conger and Brown have had a rather lengthy controversy on this subject; and apparently there is not even general agreement

on the three primary levels of matter, life, and mind. Brown makes the traditional mechanist assertion that the mental is not more complex than the mere multicellular. Brown also objects to the use of the terms "higher" and "lower" when applied to levels; he thinks the terms "more complex" and "simpler" should be used. To Alexander, Morgan, and Smuts, of course, the more complex levels are also higher in an ethical sense. Even Alexander and Morgan, founders of the theory, do not agree on the number of levels; Morgan criticizes Alexander's assertion that physical events, i.e., matter systems, emerge out of the lower level of spatio-temporal relatedness; Morgan denies that space-time exists apart from physical events. H. C. Lloyd asserts that matter emerged at the same time as mind.

As to the stage which is next higher than the human mind, this too is variously envisaged. Smuts speaks of "personality"; Alexander regards deity as an emergent quality of certain men, and his Nisus in men takes the form of a kind of urge toward divinity. In Montague's terms, this is interpreted as the ability to create children of the spirit similar to children of the flesh in the actuality of their existence, their independence, and their capacity for self-preservation and reproduction.

However, with most thinkers of this school, the next level is not deity but the human society; this is the entity which emerges from the relatedness of individual human beings. And the logic of this will be apparent, for society, or any mob, as an organism, is something more than the arithmetic total of the individual men who compose it. In the fruitful conception of society as an emergent is implicit much of materialist sociology. From the scientific point of view, the work of Wm. Morton Wheeler, an entomologist, is important. He shows that there are three biological reasons for the formation of societies: defensive, reproductive, and nutritional, and that no two societies either in the insect or in the human world, are precisely alike, each developing its idiosyncracies. There are in all of biology no genuinely solitary organisms: every one is associated at least temporarily, with some other organism, even if only with its parents or member of the opposite sex; and every organism is involved in some biocoe-

nose, or basic biological relation, with some other organism (predatism, parasitism, symbiosis, etc.). Wheeler in pessimistic vein describes the totally communalised society of ants, in which the individual degenerates not only in his social importance but also physically, and wonders whether we humans are not also approaching the same state. The history of human society records an increase in insanity, emotivity, criminality, mob psychology, and the disease of nationalism; it indicates a definite decline in hardihood and the keenness of the sense organs; and there is no proof of any increase of intelligence or the size of the cortex, in historic times. On the other hand, to other thinkers, human society is considered the prerequisite for the emergence of mind, because of the dependence of mind on such essentially societal functions as language.

The relation of the levels to each other is another factor which is not entirely clear. Morgan says that each level cannot come into being except as "involving" the levels that lie below it, and the word "involve" is used in a technical sense. Conger declares that later levels develop from earlier levels by 1) integration or creative synthesis; 2) combining relations or mutuality of relations (cf. Gestalt psychology); and 3) emergence of new qualities. The precise line of demarcation of these methods is not apparent. He also says that it is the individual variations between units on a particular level that promote their integration into units of the next level.

In the analysis of Alexander, each of the six levels after the first (space-time) is related to its predecessor as mind is to body. Thus it would not be inaccurate to refer to the secondary qualities (color, taste, etc.) as the mind of matter. Similarly, the entire cosmos is pregnant with a new "level", not yet existent but foreshadowed, which will bear the same relation to our mind that our mind does to its body. "God is the whole universe engaged in process toward the emergence of this new quality". Alexander, by the way, falls into a contradiction here, for if the new level is, like all emergents, unpredictable, how can it be foreshadowed?

In spite of ambiguities and contradictions, the theory of emergence has proved of immense philosophic utility. For example, it

provides a solution to the controversy between the mechanists and vitalists. The former assert that the peculiar behavior of living beings "can be adequately explained as the outcome of a system whose laws differ only in their degree of complication from the laws obtaining in the inorganic world" (Montague). Their case is based on three arguments: 1) since life on the earth was impossible at one time, life must have been formed out of inorganic matter; 2) biology actually progresses on physico-chemical lines; 3) any non-mechanistic interpretation would lie outside the province of natural science. The vitalists assert that "the essence of life and mind is incommensurably and inexplicably different from matter, and at least potentially separable from it, though capable of interacting causally with it" (Montague). The case for vitalism is also based on three main arguments: 1) there is a radical discontinuity between vital and non-vital phenomena; 2) biological laws differ from the laws of the inorganic world in their purposiveness; and 3) the method of biology differs from that of physics, and more nearly resembles that of history. The chief shortcoming of mechanism is that while physico-chemical laws are necessary for the explanation of biological phenomena, they are not sufficient. "Ultimately we will surely be able to 'reduce' thought experimentally to molecular and chemical changes in the brain. But is the essence of thought exhausted thereby?" (Friedrich Engels). Vitalism too has its defects, in that the "animistic factor" or "entelechy", however construed, is not spatial, and in its actions violates the universality of physical laws; besides, vitalism is inherently indeterministic and non-rational. The theory of emergence solves this dilemma by showing that living matter is due to a new structural relation of inorganic material, and that the biological level, being higher than the physical, requires not only the categories of physics but also its own categories for its exhaustive comprehension. Wheeler casts equal ridicule upon the "entelechy" of Hans Driesch, the "organisatory factors" of Eldridge, the "psychic factor" of C. D. Broad, and the "élan vital" of Bergson; the organisation and the emergence are due to the work of the parts themselves.

The theory of emergence similarly clears up a variety of other

philosophic problems, that of pan-psychism (that life has always existed in the universe) vs. external importation of life; that of the nature of the mental: is it identifiable with the neural (Sellars, Weiss) or is it a totally different type of activity? "... a mind is a material structure but has properties which could not, even theoretically, be inferred from those of its material constituents ... a mind has properties which are emergent" (Bertrand Russell, referring to Broad). The old problem as to the nature and possibilities of the interaction of mind and body is seen as a special and complicated case of the interaction of any two units of different levels. The theory offers a more precise interpretation of evolution, a more adequate conception of futurity, a more realistic philosophy of religion: the universe is constantly working itself up to higher levels, and spirit is merely a difference of level in matter. It analyzes purposiveness in psychology: "a purpose is a temporarily inhibited tendency which organises or integrates activities with reference to it" (Conger). It makes room in our logical and ontological categories for novelty and creativity. It explains the nature of freedom: "action in accordance with those characteristics which subsist at a certain level of organization but which do not exist at other (lower) levels" (Spaulding). It mediates between the positions that society is nothing more than an aggregation of individuals, and that society is something utterly distinct from its component individuals: society is the individuals-in-their-new-relatedness. "The doctrine of emergent evolution makes the biologist loyal to experimentation and observation in his own field of work ... no longer will he feel a sense of criminality in speaking of relations that are obvious in the living for the reason that they are not seen in the non-living" (H. S. Jennings). It gives metaphysical status to values: beauty is an emergent relation arising from the beautiful thing and the contemplating mind. Artistic creation is given a cosmic validity.

Certain other attributes of the theory of emergence may be mentioned here. It affords a definition of potentiality as a category which may be read into structures or processes at one level in the light of some other level. Its emphasis on things-in-relation shows its similarity of approach to that of the Gestalt psy-

chologists. The essential newness of the theory itself lies in its emphasis on unpredictability, for in no previous philosophy has this concept been so central: it is thus a kind of philosophical analogue to the Heisenberg principle of Uncertainty in the behavior of electrons.

The criticisms leveled at the theory of emergence have of course been neither limited in number nor half-hearted in fervor. Many critics, however, have found the major portion of the analysis acceptable, and merely call attention to particular defects or shortcomings. For instance, the number of levels in the universe is in great confusion. As shown before, Morgan has three, Smuts four, Brown five, Alexander seven, and Conger twenty-five. Morgan has remarked that "it is beyond the wit of man to number the instances of emergence", and truly the almost infinite complexity required of a system or catalogue of emergences on which all men would agree, militates against the value of the theory. It must be remembered that even in the camp of the emergent evolutionists themselves there is no general agreement on the three primary levels of matter, life, and mind. The general conception of a level is loose, unclear, unprecise. Whatever value it has as a heuristic tool is partially dissipated by the insistence of its protagonists, not satisfied with its utility in the philosophy of evolution, on applying it to the entire universe. An instance of what may be termed the abuse or at any rate the misuse of the concept, is in the attempt to give naturalistic status to the incarnation of God in the human flesh of Jesus.

The concept of levels meets other difficulties at other points. Brown wonders how "simpler" or "lower" can be taken to mean "earlier in time", in view of the fact that, for example, some stars are getting hotter (i.e., its atoms are disintegrating) and some stars are getting cooler (i.e., its atoms are building up to greater atomic weights). Conger calls attention to the process of "differentiation", as opposed to that of integration: molecules may be integrated from atoms, but they may also arise as differentiations within a crystal. In clarification of the exact nature of a level, he remarks that we must look beyond the mere fact of togetherness: "the test for difference of level seems to be the maintenance by the combination of units, which once severally belonged to

level *n*, of a new relatively independent individuated existence at the level *n plus 1*, with processes of interaction, perhaps reproduction and even in time perhaps new integration, proper to units of level *n plus 1*."

C. A. Baylis denies that the process of emergence is one that tends to enrich and diversify the universe, because for every instance of emergence, there is a concomitant *sub*mergence of some or all of the characters of the units which are integrated into a new whole. To take our first two examples, wetness emerges at the combination of hydrogen and oxygen, but gaseousness disappears or "submerges". There is a new quality of "chordiness" that emerges from the playing together of two separate notes; but, per contra, there is lost the penetrating lucidity which characterised the playing of either of the notes alone. For every emergence there must be a concomitant submergence; for every new thing that emerges into the universe, some known thing disappears.

Baylis also suggests the practical difficulties of applying emergent theories to actual experiments. If, for example, we have a complex *ab* that reacts in a certain manner, and neither *a* nor *b* alone can so react, we may say that this new reaction is an emergent quality of the combination *a* and *b*. On the other hand, it is possible that the reaction is due to *a* alone, with *b* or some other element, acting as a catalyst. Or the reaction may be a hitherto undiscovered property of *a*. How is one to tell, practically, whether a given phenomenon is an emergent, or a submergent, or something else?

A certain amount of the criticism cast on the theory of emergence is due to the mistaken belief that the theory asserts more than it actually does. Baylis, for example, triumphantly declares that although emergence may be necessary to establish evolution, it is not sufficient, and therefore does not really prove evolution. No one, of course, has ever maintained that emergence *does* prove evolution. Baylis also finds fault with the fact that emergence does not validate any particular metaphysics; but emergence is not so much a metaphysics as an instrument to be applied in the comprehension of metaphysics.

A more justifiable criticism is that there has not been, and

possibly cannot be, any empirical proof of many of the emergent analyses. Lovejoy and Morgan assert that secondary qualities emerge from the complex of an-object-perceived-by-a-mind. If this is to be regarded as a solution of the problem, it must be completely demonstrated.

Again, is the whole really more than the sum of its parts, as is so confidently asserted? If we consider *all* of the parts, and the interstitial filling, is there any real novelty in the complex? On the other hand, how can we ever understand the emergence of novelty in view of our subjective predicament, our being placed in the very center of things, where we can get no perspective? Which brings us to perhaps the weightiest defect in the entire theory, or at any rate, the one that most readily occurs to the reader. The emergent evolutionists say that the emergence of life on this planet is an unpredictable, inexplicable result of a particular association or relation of inorganic material elements. Is not this explanation a restatement of the essential problem rather than its solution? In explaining the novelties of emergent evolution, says Montague, we do not explain them away; he ridicules the famous "natural piety" with which Alexander said we must accept the unpredictability and characteristic incomprehensibility of emergence (despite the fact that after it occurs it may be explained in scientific terms); we must substitute for this natural piety a rational etiology. "... *rerum cognoscere causas*" —we must really *learn* the causes of things, not hypostatise our lack of knowledge of the causes of emergents into the metaphysical unpredictability of emergents. When we say that color-vision is an emergent, are we really saying anything more than that we do not *know* why the application of light of a certain wavelength, to the retina, will result in the sensation conventionally called "red"?

To this objection that the emergent evolutionists have idealised their confession of ignorance into the metaphysical principle of the unpredictability and incomprehensibility of change in the universe, Lovejoy answers, "To maintain that everything is 'explicable' in the sense incongruous with emergence, is to raise a definite, though by no means simple, question of fact; it is to imply, for example, that barring mere summations or re-arrange-

ments, there is to be found in the present phase of terrestrial history no existence whatever—no quality, type of entity, or kind of process—which could not have been discovered by a scientific angel observing the cold-gaseous-nebula stage of the development of our solar system. This proposition cannot be said to have a high degree of prima facie plausibility." In other words, *emergents are an empirical fact*, and their unpredictability is undeniable; and no one, because of this, even if endowed with genuine omniscience of every last element in the universe at a given instant, can predict the contents of the universe at any later instant. To analyse emergence is not to idealise ignorance, and to deny emergence is to assert that everything in the universe today could theoretically have been foretold a million years ago.

Lovejoy carries his defense further; he shows that any one who denies the empirical fact of emergent novelty falls into one of two fallacies: 1) reduction: the description of the emergent in terms adequate to a pre-emergent phase; this is the fallacy of behaviorism, which describes awareness in terms of molecular changes; and 2) retrotension: the assertion that the characteristics of the emergent were already present in some previous state; this is the fallacy of panpsychism, which declares that life has always existed in the universe.

In any event, Cunningham indicates that the notion of unpredictability is not absolutely necessary to the theory of emergence, and the utility of the concept of an emergent is not mitigated by the logical and scientific difficulties inherent in the idea of "unpredictability". The theory is compatible with the idea that adequate knowledge or removal from our "egocentric predicament" would reveal in advance the nature of an emergent.

Hans Driesch has attacked the theory from several points. He declares there are no emergents in inorganic matter, but only resultants; we can predict any change in matter if we know the position and velocity of its electrons; we could even predict the "emergence" of a new color if we knew enough. Of course, the "if" in each case is a tremendous word; recent research indicates that from the nature of things, the more we know about the position of an electron, the less we can know about its velocity; and it is questionable whether any amount of knowledge will enable

us adequately to predict and describe the sensation of color-vision. In organic matter, says Driesch, everything is similarly predictable, if you know the constellation of matter and the essence of entelechy. In this "if", too, lies the non-rational semi-mystical character of vitalism. Driesch also raises the egocentric predicament: novelties emerge, so far as men are concerned, but would this be true for an observer removed outside the whole evolutionary process? Phylogeny, our theory of descent, is a unique process, with an unknown end, and probably as yet unfinished; we cannot experiment on it to test the validity of emergent evolution.

Perhaps the most serious animadversion on the entire framework of emergent evolution was uttered indirectly by Dr. Oscar Riddle in his report to the American Association for the Advancement of Science in January 1936. He showed that the line between the organic and the inorganic has now become so tenuous as to be almost invisible, and that an organism which possesses part of the properties of a plant and part of the properties of a colloid, has been constructed in the laboratory. He also declared that the origin of life lay in the actions of sunlight on water, carbon dioxide, nitrogen, sugar, and other inorganic substances. The scientific transcendence of the distinction between life and matter would of course be the death-blow to the theory of emergence in evolution, but that objective appears to be an asymptote to which we draw ever nearer without the possibility of ever reaching it.

A scientific appraisal of the theory of emergence is not easy. The introduction of the concept in biological evolution is illuminating, and it has served to give at least partial answers to many a philosophic dilemma. However, like alcohol, it is a stimulant only in proper doses: many who have used it have gotten drunk in the attempt to apply it to everything. Sociology, however, is one field in which it has as yet not been applied to full advantage.

"Felix quis potuit rerum cognoscere causas", said Vergil; and if we cannot always know the causes of things, let us at least know how and why we do not know.

Emergence without Mystery

BY

WILLIAM MARIAS MALISOFF

THE meaning of emergence has been interpreted in a multitude of ways. What is it that emerges in turn from all this ado about emergence?

It is regrettable that the only distinctive overtone that seems to come forth is a lurking sense of mystery or mystification about unexpected or unforseen or "new" forms of organizations. This sense of mystery is conveyed most strongly by those who credit the alleged newness to some sort of subtle influence determined to make new things out of the old. Likewise it is also strongly suggested by those who pretend merely to stumble upon it as an "empirical" find,—but, oh, what a fantastic find!

I wish to propose a severe restriction of the term "emergence", calculated to remove this disquieting subjective sense of the mysterious. The term should be used to describe the mere fact that we can give alternative descriptions of many systems depending on the degree of organization we wish to attribute to them. This is not meant to place the notion of emergence completely under our control. It is not meant that systems do not differ in their resistance to "emergence analysis". What is meant is that in the entire situation involved in considering a system as organized, we have some control over the choice of points of view or "levels" of description. Thus, we can describe a human being at the physical level, the biological level and the psychological level, as we please, albeit not with equal success. I maintain that when we do that, the sense of mystification seems to evapo-

rate, first, as an observed, experimental fact on many who have taken up the view in detail and second, as reasonably so because we have retained *control* and are not nonplussed by and rendered completely helpless before an absolute novelty.

The proposed restriction is best described as a choice to order "levels" according to the number of postulates required to specify them. When we note that a "higher" level has appeared we have added new postulates to our mode of description. We are still free to ask why a new level has appeared in "nature", or why such progressions occur, or why more or less suddenly we *have to* add postulates, but we have lost the particular sense of mystification I wish to combat. One may say that the problem of becoming is thereby dissected away from the problem of comparison of organization. I believe it is helpful to do so, if only to isolate the confusions in one region, a region of speculation concerning ultimates, beginnings, and finalities, such as one encounters in a vague, unqualified contact with "becoming".

How levels can be set up may be shown by an arrangement of the sciences according to the number of postulates required to characterize them. Since the postulates have not been worked out, the list is clearcut only when we pick out the outstanding trio, physics, biology, psychology. Biology deals with systems which are simultaneously physical; therefore, it contains at least all the postulates of physics or of a physics, plus its own characterizing or "emergence-producing" postulates. Psychology deals with systems which are simultaneously physical and biological; therefore, it contains at least all the postulates of a biology, plus its own characterizing postulates. If mathematics is to be used in these sciences, they must contain the postulates of the particular form of mathematics chosen. The various forms of mathematics themselves can be arranged on a scale of complexity as to the number of postulates. The lower the number of postulates in a mathematical system, the more general it is. Indeed, generalization may be described most "generally" by noting that generality is increased as the number of necessary and sufficient postulates is decreased. Thus, a well-known mathematical system with 8 postulates is more general (or is at a "lower level") than the 27

postulate system, containing the same 8 as a sub-set, required for the algebra of complex numbers (which is at a "higher level"). Within physics there are also systems of varying generality, i.e. requiring different numbers of postulates. In every case the lower level can be transformed to a higher level of lesser generality by the addition of necessary postulates, rendering the first a sub-set of the second. Kinematics is distinctly more general and at a "lower" level than other branches. The same applies to relativity, and probably to quantum mechanics, as against the physics of deformable bodies which have "histories" and require more for complete specification. Levels of postulational complexity will appear from such work as Woodger's in biology, in which a sincere effort is made to find an axiomatic basis for the science. A group at Yale is trying the same thing for psychology. It is predicted here that both in biology and psychology these workers will probably discover a further stratification of their sciences according to the number of postulates taken, as is already apparent in physics, and has been a fact for a long time in mathematics.

The argument can now be permitted to return for a while to the question of why the exposition of the *logical structure of emergence* removes the sense of mystification one gets from the qualitative and vague illustrations cited to demonstrate absolute "novelty", "creativeness" or similar psychological magic. It does so because it is really illuminating and holds out hope of quantitative treatment. The illustrations usually given by the mystics diverge in two directions; one runs to the most colossal problems of organization, the other to the trivial issues of combination. One kind of mystic asks: how could one have forseen from a mere knowledge of chemistry and physics the possibility of LIFE? or, further on, the appearance of MIND? Another proposes: put the gases hydrogen and oxygen together and with a little coaxing they will form water, but, mark you, the water has *absolutely novel* characteristics such as *wetness* that the gases did not have. Life, mind, wetness could not have been predicted. They contain the priceless ingredient which makes the whole in those cases greater than the sum of the parts. So stated the

illustrations do produce an animistic response. One can even get that out of the proposition: one and one are two. Merely insist that the property of being an even number has been added to that of being odd and prime. There emerges the first and only *even* prime number! One can see startling novelty in this when one is in the right mood. All I wish to stress is that the mood is the thing. When the Chinese sage said that a horse and a cart are three, he almost destroyed the mystery by salting it with humor. The destruction can be completed by showing that a horse and cart are many, if one considers that the horse may be before the cart, behind the cart, beside the cart, inside the cart, any distance from the cart, all over Euclidean space, possibly in a ghostly state depending on the time, possibly Eohippus, possibly a horse of a different color, possibly anything you may wish the words to mean.

The stock illustrations lose their mystery before any quantitative or specific attack. "Wetness" to the chemist as chemist is not the psychological sensation. At his level of discourse it is definable in physical terms and is predictable. Furthermore, he thinks of the reaction of the formation of water as reversible. Thus, gaseousness is quite as emergent as wetness, and one is no more mysterious than the other. In fact, historically, the surprise came in the decomposition and not in the synthesis of water. The moral is: don't mix psychology and chemistry indiscriminately. Taken psychologically the atom and the molecule are on a par, or possibly the former is more complex (higher) than the latter. Taken within the framework of chemistry, the molecule is the more complex or the higher, and even there it is that by virtue of the postulates of chemistry.[1]

Many emergents will turn out to be "resultants" on closer examination, to the degree one guards against mystification.

The illustrations that point to the major issues are harder to de-mystify. Yet biologists who keep on asking what life is to

[1] I do believe that with some skill in intellectual acrobatics the molecule may be taken as the simpler. Indeed such a situation exists without forcing in physics in connection with neutrons and protons. *Each can be synthesized from the other* (neutron + positron = proton and proton + electron = neutron). Which is the more complex?

dramatize the subject in the classroom, proceed in the research laboratory as if it were a resultant and not an emergent, or let us say, as if it were an *emergent without mystery*, a materialistic emergent from start to finish, allowing no ingression of that which is not in space-time. Even when they speak of organizing *factors* they look for them in a material something. Thus, Spemann found "the organizer," a stable complex substance which dominates cell development. Logically, to speak of an organizing factor, is to make a demand for the addition of a postulate or postulates (verified by the peculiar structure and relationships of the "organizer") necessary to define a developing system. Experimentally, if the parallel is correct,[2] the addition of the postulate is carried out by the addition of the organizer of a developing cell to the protoplasm of another kind of cell which has been deprived of its organizer, and actually making it develop along the lines dictated by the new organizer. With all the necessary conditions (= postulates) for a particular development, that development takes place.

My own illustration suggests a happy coincidence between the behavior of matter and meaning. The logical structure of emergents, however, requires no such strict parallelism. This looseness permits a new situation, a new construction, a new set of postulates to emerge all over, giving the new the character of a whole.

The liberating effect of arranging systems by the study of the relations between their postulates is seen from the following. Imagine sets of mathematical postulates in increasing order of complexity, M_1, M_2, M_3 and so on; imagine similar sets of purely physical postulates, P_1, P_2, P_3 and so on; and also a set of purely biological postulates, B_1, B_2, B_3 and so on; and also a set of purely psychological (consciousness) postulates, C_1, C_2, C_3 and so on. The possible complete forms of physics will be those consisting of sets of postulates, $M_1 + P_1$, $M_1 + P_2$, $M_1 + P_3$ etc., $M_2 + P_1$, $M_2 + P_2$, $M_2 + P_3$, etc., $M_3 + P_1$, $M_3 + P_2$, $M_3 + P_3$ etc. and so on to as many sums as can be built up. If the total number

[2] I do not claim that the parallel *is* so close, but the experiment almost begs for such an interpretation.

of purely mathematical postulate sets is m, and the total number of purely physical postulate sets is p, the total number of possible complete forms of physics will be $p\ m$. The reader can see that the same can be done with purely biological and psychological postulates, and that if the number of the former is b and the number of the latter is c, the total number of possible complete psychologies will be $cbpm$. This can be a very large number indeed and can include many queer forms. For instance, we might have a psychology restricted to a few integers, physical identifications by contiguity, admitting only the simplest biological and psychological functions. Many forms will not find exemplifications. Perhaps none will ever be verified even to the extent of the scientist's modest requirements.

That brings us to the question of the relation between actual and possible emergents. The last refuge of mystery is in the unadulterated actual. The flux which has produced everything, including us, in a process of organization and integration shows emergence-in-general. In all this our private attitude toward the passage of time, taken on the wing, may be legitimately quite mystical. It is emotionally very satisfying to lose oneself in the intimacy of the actual-as-lived. But it is impossible to carry on any science or inquiry of social import without setting up contexts, limits, special points of view, and logically, if they could only be expressed, postulates. The postulates, of course, are not explicit. My hope is that some day they will be made such. I believe they are implicit, or that at least it is a good procedure to look for the implied postulates back of any description. Once we enter upon the path of science we must deal with the possible. The actual then becomes a goal or an ideal to which the possible explanations converge indefinitely. Many deviations and errors must be uncovered on the road to truth-as-an-ideal. What we think are definite emergents may also be in error.

No longer, then, can we attribute any complete fixity to emergents. Having proposed that they (the possible ones) partake of the nature of scientific constructs, I must insist that they can retain no more or no less permanence or distinctness of outline than anything scientific. If causality admits of an admixture of

indetermination elsewhere, it will do so as well in the case of the appearance of emergents. The very multiplicity of possibilities permits of an evolutionary movement which need not be mechanistic. Passage occurs within the field of possibilities. The mechanistic passage is only one kind, and a very special kind, of passage among possibilities. Emergence need not be mechanistic or strictly deterministic.

I am equally opposed to absolute prediction and to absolute non-prediction, which many philosophers of emergence seem to require. Emergents reside in the realm of *probable* knowledge.

I would admit unverifiable emergents as possibilities, empty of empirical content, but possibly valuable in some way as being in the same formal family with those which have received verification. There have been too many cases where an unverifiable turned out to be verifiable after all. It is only by a complete knowledge of our postulates that we could set up something unverifiable-in-principle (or unobservable-in-principle) and even then it would be so only relative to our postulates. The negative states of Dirac play some such rôle in physics. They have, however, some full blooded kinsmen-by-mathematics, and can thus justify their ghostly presence.

The conventional character that attaches to all choices is further illustrated by the constant breakdown of "natural" systems of classification in science. This is a sign, I think, of progress. Emergents, like species, keep fading as missing links are filled in, till one is forced to coin expressions like 'continuous variability', with a fear that even the stable interbreeding limitations of heredity may break down. The genes show alarming indeterminacy. The viruses are also perplexingly indeterminate. These are but reminders that convention is not absent in the defining of emergents. Hordes of false opposites produce false emergents.

Emergents are relative in the typical sense of being subject to the mode of observation, i.e. relative to specifications and presuppositions or postulates. Thus, the dimensionality of "space" (Alexander would call it an emergent) is distinctly relative. With regard to points it is 3; with regard to spheres, 4; with regard to

forces, 6; with regards to solids, 6. Here is something which is 3,4,6-dimensional, depending on how you look at it.

Within all this relativity something invariant remains, namely the mode of construction of all spaces. Our search should be for just such invariance. Emergents, too, will show some character of invariance, a character common to all emergents. The necessity for postulate delineation is only a part of the invariant character. There are still numerous problems connected with emergence which require reinterpretation in this light: the necessity of conservation (in physical terms) or the equivalence of the cause and effect (in logical terms); the blending of the "found", the "given" and the "taken"; the persistence of classes. In all cases we must ask: what is it that is relative and what is it that is invariant?

For the present I have limited myself principally to an examination of the logical structure of emergence. I have also touched upon emergence as it appears in all its materialistic and relativistic uncertainty in the empirical research situation. I have mentioned some conventional aspects which I believe lead further back to considerations of semantics, such as a more or less free choice of contexts and even of the logic to be used in identifying emergents. The unstressed points of view will receive more elaborate treatment later. This can be given with less mystification once the logical character of emergence has been freed of irrationality, as I hope has been done in this communication.

New York, N. Y.

The Structure of Wholes[1]

BY

ANDRAS ANGYAL

I

IN ATTEMPTING to clarify the problem of personality integration the writer gained the impression that the difficulty of such a task does does not lie alone in the paucity of usable factual data but it is due, even to a greater extent, to the inadequacy of our logical tools. Such a handicap is felt not only in the study of personality, but in the study of wholes in general. Here the attempt will be made to develop some concepts which may be useful for the understanding of the structure of wholes.

Our scientific thinking consists prevalently in the logical manipulation of relationships. That the structure of wholes cannot be described in terms of relationships has, however, been repeatedly pointed out. Along with the insight that holistic connections cannot be resolved into relationships it was also implied that the pattern or structure of wholes does not lend itself at all to logical manipulation. It is possible, however, that the structure of whole is amenable to logical treatment, that it can be described in logical terms, although not in terms of relations, but in terms of some more adequate logical unit. This unit may not be reducible to relationships, but it may represent an entirely different logical genus. Here the attempt will be

[1] The following discussion forms the first section of the chapter "Personality Integration" in a monograph entitled "Foundations for a Science of Personality" which is being prepared for publication.

made to demonstrate that there is such a logical genus which is fit for the treatment of wholes; we propose to call it "*system.*"

The ideal would be to develop a logic of systems to such a degree of precision that it might offer the basis for exact mathematical formulation of holistic connections. As A. Meyer states, "The mathematics which would be needed for the mathematical formulation of biological laws does not exist today. It has to be created by the new biology."[2]

To construe a logic of systems as a counterpart of the conventional logic of relations is in itself a gigantic task and it cannot even be attempted here. It may be true that a substantial advance in the study of wholes and in the study of personality specifically will come from the development of a logic of systems. We must, however, content ourselves for the present with the clarification of some aspects of the logical properties of system and with applying the insight which may be thus gained to our specific subject matter.

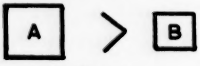


FIG. 1

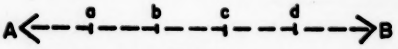


FIG. 2

In order to demonstrate some of the logical characteristics of systems we may compare them with better known logical forms, namely with relationships. As an example of a relationship we may take a quantitative one (See Figure 1) and as an example of a system we may take a simple geometrical one (See Figure 2) in which the points *a*, *b*, *c*, *d*, are parts of a simple geometrical system of "linearity" (AB).

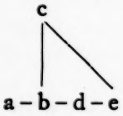
The differences between relationships and systems may be formulated in the following points.

1. A relation requires two and only two members (relata) between which the relation is established. A complex relation can always be analyzed into pairs of relata, while the system cannot. A system may involve an indefinite number of members. A system is not a complex relation. It is impossible to

[2] A. Meyer, Ideen und Ideale der biologischen Erkenntniss, Leipzig, 1934 (p. 35).

say what the relation between *a* and *b*, *b* and *c*, *c* and *d*, etc. should be in order that they form a linear system.

I am aware of the fact that the restriction of the term "relation" to cover only two-term connections deviates from the contemporary usage of this term. Usually also such logical connecnections which involve more than two terms are spoken of as relations. Such connections, however, seem to fall into the following two categories: (a) Compound relations which can be reduced to two-term relations. One or both of the relata may be groups involving a more or less large number of members. Group A may include *a*, *b*, *c*, *d* and group B may include *e*, *f*, *g*, *h*. When, however, group A is related to B, or to one member of B, e.g. *f*, the group is taken as a totality, that is, as *one* unit. A compound relation may involve also a chain connection (e.g. a causal chain): $a - b - c - d$. It is clear that such compound relation can easily be resolved into two-term relations: $a - b$, $b - c$, $c - d$. A more complex relationship would be the following:



which consists of the two-term relations: $a - b$, $b - c$, $b - d$, $d - e$, $c - e$. The complexity may be even greater and a reduction to two-term relations still is possible. (b) There are also connections which involve more than two terms and cannot be reduced to two-term relations as, e.g.: *b* is between *a* and *c*. It appears, however, that those compound connections which cannot be reduced to two-term relations, exhibit all those qualities which are claimed in this paper to be the characteristics of systems. One may, of course, use the term "relation" in the very broad sense as it is commonly done, but then one must admit that one subsumes two very different logical genera (two-term relationship and complex connections which are reducible to two-term relations on the one hand and complex connections which cannot be reduced to two-term relations on the other hand) under the same term.

Also the term "system" as used in this discussion is at variance with the common usage. Usually one designates as system any aggregate of elements considered together with the relationships holding between them. It will be shown in the following discussion that the type of connections in a whole is very different from the connections which exist in an aggregate. The term "system" is used here to denote *holistic systems*. Further, in using this term we abstract from the *constituents* ("elements") and refer only to the *organization* of the whole. Thus system is for our discussion holistic organization.

It might seem desirable to substitute other terms for "relation" and "system" in the present discussion; however, it seems to me equally desirable to avoid the coining of new terms. Since I have pointed out the differences between my usage and the common usage of the terms I may hope that the argument will not become obscured through this terminology.

2. A relation requires an aspect out of which the relationship is formed. Two objects can be related to each other, for instance, with regard to their color, size, weight, etc. Therefore, before a relationship can be established it is necessary to single out some aspect of the relata which serves as the basis of the relation. The attribute of the relata on which the relationship is based, is an immanent quality of the object, like size, color, weight, etc. The object enters into a relationship with another object, thanks to its immanent qualities. Most relationships are based upon identity, diversity, or similarity (partial identity with partial diversity) of the objects, that is, on immanent attributes. If both relata can be resolved with regard to the attribute in question into identical smaller units we have quantitative relations; otherwise the relation will be a qualitative one. The members of a system, on the contrary, do not become constituents of the system by means of their immanent qualities, but by means of their distribution or arrangement within the system. The object does not participate in the system by an inherent quality but by its *position in the system*. It is immaterial for a linear system whether stars or crosses or circles or whatever objects are the members, if only in the arrangement the positions remain the same.

Between the constituents of a system, after they gain a positional value from the system, *in a secondary way* also relations may be established which are not based on the immanent attributes of the relata, but on their secondary positional value. Such a relationship is, for instance, A is below B. Such relations are secondary: it is presupposed that the members have a positional value in a system of co-ordinates.

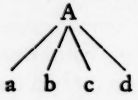
3. The presupposition for establishing relationship between objects as well as for arranging objects into a system is the separation of the objects. Multiplicity of objects is only possible in some kind of dimensional medium. The clearest examples of dimensional media are space and time which have been recognized by philosophers for a long time as "principia individuationis", that is, media which make possible a multiplicity of individual objects. We cannot speak of *two* objects unless they are placed in different points of time or in different points of space or a distance between them is established in some other kind of dimensional medium.

Although the dimensional medium is a necessary condition for both relationships and systems the function of dimension is different for these two logical genera. The rôle of the dimensional medium for a relationship is merely disjunction of the relata. But the rôle of the dimensional medium ends here. The medium itself does not enter into the relationship. For instance, if two colors are separated in space a comparison between them can be made without any further reference to space.

The dimensional medium is more intimately involved in the formation of systems. Here the dimensional medium not only separates the parts, but it participates in the formation of the system. The system itself is dimensional. *Systems are the kinds of distributions of the members in a dimensional medium.*

4. In a relationship the connectedness between the relata is a *direct* one. The connection goes without any mediation directly from *a* to *b* and vice versa. The connection between the members of a system is, however, of an entirely different type. Although there is a connection between the points *a*, *b*, *c*, *d*, when they form a straight line, this connection is not a direct one. It is impossible to say what the relationship between *a* and *b*, between *c* and *d*, *a*

and *d*, etc., should be to form a linear arrangement. The members of the system are connected by the mediation of the whole, by the mediation of the system itself. System connectedness of the parts cannot be expressed in such a way as: $a - b$, $b - c$, $a - c$, etc., but



In a system the members do not hang together among themselves but they hang together in the whole.

The constituent parts of a system always point beyond themselves; they point toward a superordinate, more inclusive factor,

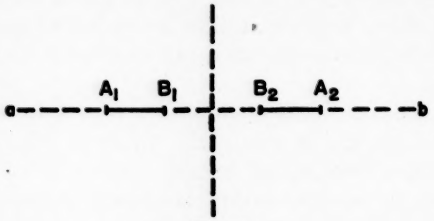


FIG. 3

the system in and by which they are connected. An interesting example of this state of affairs is given in the fact of geometrical symmetry. A symmetrical arrangement involves two figures which are identical in shape and size and have inversed positions in space. The identity of geometrical figures can be demonstrated by superposing them. Symmetrical figures, however, can be superposed only under special conditions. Taking one-dimensional geometrical figures such as those in Figure 3, it is clear that such figures cannot be superposed by moving them in a one-dimensional space, that is, along the line $a - b$. A_1 will not coincide with A_2 and B_1 with B_2 at the same time. In order to superpose them one needs to move them within a two-dimensional space. Thus the position of the two lines is "inverse",

not in a one, but in a two-dimensional space. Taking two dimensional symmetrical figures such as those in Figure 4, it can be easily seen that they cannot be superposed in any way, if one shifts them within a two-dimensional space, but only within a three-dimensional space. Three-dimensional symmetrical objects again cannot be superposed in a three-dimensional space. They would require a fourth dimension. If one's mirror image were real and one could go beyond the mirror, one still could not step into one's mirror image and completely overlap with it, because the right side of the person would be on the left side of the mirror image and vice versa. For the superposition of symmetrical figures a space which has one dimension more than the number of dimensions of the symmetrical figures is required. Thus one-dimensional symmetrical figures are inversely placed

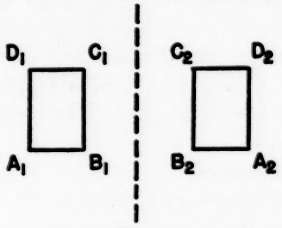


FIG. 4

in a two-dimensional space, two-dimensional figures in a three-dimensional space, etc. Symmetry exemplifies the system connectedness, that is, a connectedness of parts, not between themselves, but in a superordinate whole.

Up to this point we have considered only one type of relations, namely comparative relations based on identity, diversity and similarity. Another type of relationship which is of great significance in science is the causal relationship. Causal relationship can be expressed as follows: if there is *a* there is also *b*. This is a relation of necessary dependence. Causal relationships may be in many respects different from comparative relationships, but they are alike in the respect that they represent direct connections between one member and another. Even complex

causal relationships can be analyzed into two-membered relationships. Where such analysis is logically not possible, we deal with system connectedness.

On the basis of the foregoing discussion the differences between relations and systems can be summarized in a preliminary fashion as follows: (1) Relationships involve two and only two members (relata). Complex relationships can always be analyzed into pairs of relata. Systems may involve an indefinite number of parts. (2) The relata enter into a relationship primarily thanks to their inherent attributes, while the constituents enter into a system connection, not with their immanent attributes but with the position which they occupy in the system. Secondary relations which are based on positional values of the relata, can be established also between members of a system. But the system itself cannot be described even in terms of such relationships. (3) For the existence of systems a dimensional medium is necessary. Systems are specific forms of the distribution of members in a dimensional medium. (4) In relationships the connection between the relata is a direct one. The members of a system, on the contrary, do not need to be directly connected; they hang together through the whole.

Dealing with relations and dealing with systems involves two different logical manipulations to which also two processes that are quite different psychologically may correspond. In the recent past there has been much, but rather inconclusive discussion as to the possibility of two different processes of knowing: explanation and understanding. I am referring here to the discussion around the problem "erklärende und verstehende Psychologie." The difference between the two concepts, as they have been used in the aforementioned discussion, is probably that explanation refers to relational thinking, understanding to system thinking. Relational thinking aims at the establishment of the direct connection between two objects. For instance, in the study of causation one has to find to member *a* (effect) a second member *b* (cause) with which it is connected by necessity. In causal research the task is to single out from a multiplicity of data pairs of facts between which there is a necessary connection.

In system thinking the task is not to find direct relations between objects, but to find for the members the superordinate system in which they hang together or to define the position of members relative to the subordinate system.

The preceding discussion is far from being complete and adequate, but it is hoped that the thesis has been made at least probable, namely, that relations and systems are two different logical genera with distinctive characteristics. It is still an open question whether relations and systems are absolutely different logical genera or whether the one may be a subtype of the other. The latter possibility cannot be excluded and there are certain arguments in favor of it. One thing, however, seems clear, namely that systems cannot be deduced from relations, while the deduction of relations from systems still remains a possibility. If that is the case then the more general logical genus would be "system," while "relation" would be a reduced, simplified system which is adequate only for the logical presentation of very simple specialized constellations.

II

In the course of the past two decades it has been almost generally recognized by biologists and psychologists that the clarification of the problem of wholes is essential for progress in the study of the organism. The increasing awareness of the problem of wholes led to the discovery of some general principles; such general principles have been perhaps best formulated by the Gestalt psychologists. It will be useful to examine briefly these formulations in the light of the previous discussion.

The most generally known thesis with regard to wholes is the following: "The whole is more than the sum of its parts." This is not a very felicitous formulation since it may suggest that a summation of parts takes place and *beside* this summation a new additional factor enters in the constitution of wholes. In Ehrenfels' "Gestaltqualität" such an additional factor actually has been suggested. Wholes are, however, quite incomparable with additive aggregations. Instead of stating that in the formation of wholes something more than a summation of parts takes place

it would be more correct to state that summation does not play any part whatsoever in the formation of wholes. In summations the parts function thanks to their inherent qualities. When, for instance, linear distances are added to form a larger linear distance the first distance as such is directly joined ("Und-Verbindung") to the second distance and this to the third, etc. On the other hand, when a number of parts constitute a whole, the parts do not enter into such connection by means of their inherent qualities, but by means of their position in the system. The formation of wholes is therefore not additional to the aggregation of parts, but something of an entirely different order. *In aggregates the parts are added. In a system the parts are arranged.* It should also be kept in mind that "part" means something different as applied to aggregate and as applied to wholes. When the single objects *a*, *b*, *c*, *d*, are bound together in an aggregate, they participate in that aggregation as *object a*, object *b*, object *c*, etc., that is, as lines, distances, color spots, or whatever they may be. When, however, a whole is constituted by the utilization of objects *a*, *b*, *c*, *d*, the parts of the resulting whole are *not object a*, object *b*, object *c*, etc. but α, β, γ, δ, that is, the *positions of the objects a, b, c, d.*

I would suggest that the principle "The whole is more than the sum of its parts" be modified in the following way: Aggregation and whole-formation are processes of an entirely different order. And we may also formulate this statement more concretely: In an aggregation the parts are added, in wholes the parts are arranged in a system. The system cannot be derived from the parts; the system is, so to say, an independent framework in which the parts are placed.

That the whole is to a large extent independent of the individual parts has been frequently pointed out. We may transpose a melody an octave higher or lower and it still remains essentially the same melody although this transposition may be such that the two variations of the melody have no single individual tone in common. If we recall that the system is a kind of arrangement in which the parts do not participate by means of their inherent characteristics, but by means of the positions, the above-men-

tioned independency of the whole from the nature of the individual parts will be understandable.

The above statement, however, needs some qualification and restriction. The parts may need to have certain attributes which enable them to fill the positions which are required for the system. In a triangular geometrical arrangement the parts have to be *lines* although the other properties (e.g., their absolute length) are relevant. Thus certain properties of the constituents are relevant; that is they are necessary for being able to occupy a given position, while other properties are irrelevant. A similar distinction has been made also by J. von Uexküll, who differentiates between "leading" and "accompanying" properties.[3] The greater the organization of the whole the more the inherent properties of parts are utilized as co-determinants of positional values. The human organism, e.g., is highly economical in this respect: it carries a minimal load of irrelevant properties of parts; most of the properties of parts are "utilized," i.e., are co-determinants of the positional value of the part.

"Wholes are never undifferentiated but always a *unitas multiplex*." Let us place the emphasis first on the multiplicity. If one keeps in mind that the system is a way of arrangement of parts, the logical necessity of a multiplicity becomes evident, since a single factor in itself cannot be arranged.

The term "whole" is frequently used in a very confusing double meaning. Sometimes the concrete organized *object* and at other times the *organization* of the object is called a whole. The term is used in this latter connotation, for instance, when one states that a circle may be small or large, drawn in red or in green color and it still remains the same whole, the same Gestalt. I propose that the term whole be reserved to designate the concrete *organized object* while the *organization* itself, the way of arrangement of parts, should be called system.

The logical formulation of a given system states the construction principle or the *system principle* of the whole. Every system has one and only one construction principle. This is the meaning

[3] J. von Uexküll, Theoretische Biologie, 2nd ed., Berlin, 1928 (p. 87).

of the first term in the expression "unitas multiplex." The particular system principle may be perfectly or only approximately realized in a given whole. There are wholes in which all the significant positions of the system are occupied in perfect accordance with the system principle, but there are also wholes where only a limited number of positions, sufficient to suggest the system principle, are occupied while other members are out of position. This is the difference which among Gestalt psychologists is somewhat vaguely referred to by the terms "good" and "bad" Gestalts. The various degrees of "pregnancy" which a Gestalt may have express the degree of conformity of the position of the parts with the system principle. There are also instances where, in a whole, a sufficient number of positions are occupied to indicate the system principle while the other positions are not filled. These are "open" Gestalts in contra-distinction with "closed" ones where all the significant positions are occupied.

Certain relations, for instance, comparative relations, (e.g., *a* is larger than *b*) could be called static while others could be called dynamic. The prototype of the latter is the causal relationship. In the same fashion one could distinguish between static and dynamic systems. We hope that we have demonstrated the existence of the static form of systems. The question, however, as to the existence of dynamic systems still remains open. In static systems the whole imparts to its constituents a positional value which the given constituent does not have in itself, but only when it forms part of the given whole. With regard to dynamic wholes, one would expect that the parts *function differently* when they form part of one or another whole. We would also expect that the whole has its own characteristic dynamics. Certain principles of holistic dynamics have been already formulated as, for instance, the "tendency to closure" and "tendency to pregnancy." Such dynamic principles have been up to the present date satisfactorily demonstrated only within the psychological realm which is generally characterized by great plasticity. That such principles hold true also in realms of greater rigidity is strongly suggested by a number of facts, but is far from being satisfactorily demonstrated. Such assump-

tion is only a working hypothesis and a convincing demonstration of it remains for the future.

The possibility of a dynamic action of systems would be probably rejected a priori by many students. Although causality is in the last analysis just as unexplainable as a system-action, still many students would feel more comfortable and they would be willing to assign a greater credit of scientific validity to the formulation of the dynamics of a given happening in terms of causality rather than in terms of system-action. Causal thinking has been used in science for such a long time and in certain fields with such success, that it has been almost generally considered as *the* scientific thinking, although it may be well only a sub-variety of it. The relational thinking is such a firmly rooted habit that the transition to system thinking is at least as difficult as the transition from a three—dimensional to a four—dimensional geometry.

This brief discussion of some holistic principles and concepts suggests the possibility of a logical formulation of such principles and concepts. Only strict logical formulations can dispel the vagueness and obscurity which have been so common in the early holistic theories.

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The Philosophy of "As If" in Physical Science

BY

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WE ARE accustomed to think and to speak of the physical universe as having objective existence, externally to ourselves; and as being in objective existence quite apart from any question of our perception of it. Even the idealistic philosopher regards the universe so, except when he is reflecting mentally upon other possibilities. The realistic view is the primary view. Idealism is an afterthought. And the idealist, if he is an investigator in physical science, cannot avoid acting upon the postulate of realism. Nothing else gives him a foothold for work. He has to regard the universe "as if" it were real. All our philosophies are necessarily based upon postulate; all our science is founded upon faith.

Mirage occurs "as if" light were reflected from the surface of a pool of water lying on a roadway, whereas it is really refracted by a layer of hot air close to the road which is heated by the rays of the sun. But the mirage looks real until it suddenly disappears when the relative position of the observing eye is sufficiently changed. A real pool of water acts differently. Though this does not prove the non-subjectivity of the real pool, it does prove the existence of two classes of things, apparently alike, and the strength of the contrast suggests a strong opposition as to conditions, such as is specifiable by the terms subjective and objective. The adoption of either view has to be regarded as a *postulate*,

each being equally possible, although there is an aspect of the fantastic in extreme solipsism. Even the solipsist has to treat the physical universe "as if" it had objective existence.

That is a feature of scientific theory which has always to be kept in view; not that the "as if" should be expressed perpetually, but that it should be held in perpetual remembrance, for the oversight of the condition is a prolific cause of misunderstanding, more especially in popular writings. The realist cannot disprove the tenets of the idealist; nor can the idealist prove the realist to be wrong. Consequently the omission of the limitation, in statements about the physical universe generally is not of such deep disadvantage as it is, or may be, in more special cases. The philosophy of *As If* is not a philosophy of Agnosticism; it is a philosophy of Investigation, or rather of a foundation upon which enquiry can be based.

The condition *As If* is wider than the condition *If*. For the latter is specific and definite, while the other does not necessarily exclude a different illustration. Thus we may, from some experimental points of view, say that electricity in a conductor flows as if it were an incompressible fluid. From another point of view we can say that it acts as if it were a perfect gas.

The assertion "as if the condition A exists" does not imply that A really holds. It allows the postulate to be made as a working hypothesis. But inadvertent omission of the prefix "as" makes the postulate definite, and may give historical trouble. On the other hand, inadvertent omission of a condition "if, &c", in any scientific statement, may make the residual statement false.

Newton's law of cooling was stated by him with great care and entire completeness. But, in one textbook, one given condition was omitted, and other textbooks copied the error until the correct statement became practically unknown. The result followed that Newton's law was regarded as a very poor approximation. So it seemed to form a most extraordinary exception to the fact that modern repetitions of Newton's experimental work only succeed in verifying its remarkable accuracy. Towards the end of last century, Dr. Crichton Mitchell, working in Tait's

laboratory, discovered and rectified the omission. Newton's condition "in a steady uniform current of air" had been dropped. His law in fact is not a law of cooling by radiation alone. It is a law of cooling by radiation and powerful convection.

A similar condition appeared in connection with Kelvin's determination of the age of the earth. At the time of his calculation only one sufficient source of the earth's internal store of heat was known; and he showed, by conclusive dynamical reasoning, that the resulting age of the earth could not exceed a few hundred millions of years. When the heating effect of the radioactive substances present in the rocks forming the crust of the earth became known, a large extension of the age of the earth, sufficiently large to satisfy geological specialists, became available. At once an outcry arose regarding the supposed failure of Kelvin's dynamical reasoning and conclusions. There was no failure. Sir Oliver Lodge pointed this out, and the clamor at once ceased. Although, at the time of Kelvin's work, there was not the faintest idea of the possible existence of any other sufficient source of heat than that considered by him, nevertheless, careful as Newton, Kelvin prefixed his calculation by a clear statement of his postulate—If no hitherto unknown source of energy exists. No one knew better than he that assumption lies at the root of all philosophy. His perception of a possibility constituted practically a successful prediction.

This subsequent transformation of a stated *If* into a positive assertion can give rise to prolonged detrimental consequences. One illustration will be sufficient, for it contains four successive examples. These arose in the development of the trichromatic theory of color vision. Young's original, purely illustrative, postulate of the existence of three distinct types of retinal nerve endings, each originating a corresponding type of color sensation, was interpreted as a positive assertion. The absence of any anatomical distinction of that kind was regarded as disproving the theory. A similar fate befell Helmholtz's suggestion, also explicitly tentative, of the possible existence of three types of chemical substances in the nerve endings. Yet it had been pointed out with elaborate clearness that the truth or falsity of

the trichromatic theory was in no way dependent on the particular origin of the trichromatic condition. Again, Young's original hypothetical assumption, that dichromasy was due to lapse of one of the three fundamental sensations, was found to be inconsistent with observation; and again, in some quarters, the theory was supposed to be invalidated. But Helmholtz merely substituted the next simplest hypothesis for that of Young, which procedure met the needs; and he finally gave entire generality to it, so that it could cover a field far wider than that actually known. Lastly, Helmholtz's simplest assumption of the intensities of the component stimuli as the coördinate quantities in the color field, was, after his death, found to be inconsistent with Abney's law. W. Pauli, Jun., pointed out that the square roots of the stimuli should be chosen as the coördinate quantities in order to give agreement with Abney's law. This condition was adopted by Schroedinger and was carried out by him to the final establishment of the trichromatic theory. But the history of its development has been one of repeated unintentional misrepresentation or mistake, and of wrong views of the necessities or permissions laid down.

A marked example of the omission of a stated "as if" condition occurred in connection with Kelvin's great work on the luminiferous ether. That work constitutes one of the finest examples ever given of the persistent sapping of difficulties which beset theory, and of the carrying forward of the theory, through years of labor, to an entirely successful issue in so far as known data at the time were involved. The fact that a new difficulty arose immediately thereafter in no way detracts from the triumph. Yet Kelvin was misrepresented as having said that the ether *is* an elastic solid. What he did say was something very different. He said that in so far as the vibrations of light are concerned, the ether acts *As If* it were an elastic solid. And he actually added "I do not say that it is an elastic solid". Moreover he proceeded to show how, regarded from another point of view, it could not be an elastic solid. But his quasi-contractile ether acted in so far as the vibrations of light were concerned as if it were an elastic solid. And, further, it nevertheless offered no resistance to the

passage of matter through it. Kelvin's problem must some day be returned to.

The question presses strongly today—What are the "Ifs", "As Ifs", and "In so fars", stated or unstated, which relate to current physical speculation?—the two outstanding examples therein being Relativity theory and Quantum theory.

Is the ether a necessity? Or is it gone for ever, as some extremists hold?

It might be well, in dealing with this point to ask if the idea, that the ether wave equations do not indicate actual oscillations of some real thing, had its origin within the present century? If we look back three quarters of a century, we find Kelvin saying "What is this ether, and how are these geometrical undulations really produced? Are they more than abstract or *Mathematical Entities*, indicative perhaps, in its highest empirical form, of some grand ultimate law? It cannot be denied that great physical difficulties exist. . . . ." So he set himself to overcome the difficulties, but not by denying the reality of the ether. When we have found something that acts in accordance with physical requirements, we are justified in introducing an "as if" in describing the action. The subsequently appearing difficulty, alluded to above, originated the theory of Relativity, and gives rise to a variation of Kelvin's question in the form "Is Fitzgerald's contraction real?"

What is space in itself? Perhaps the best definition is that involved in the request of the worthy Edinburgh bailie, who, when superintending school games, found the boys crowding the competitors too closely, and asked them to give "*room for scope*". But room for scope is not itself enough in generalised relativity. The room has to sustain twistings in order to compel matter to follow curved geodesic paths. As Sir J. J. Thomson once concluded in a broadcast address on the subject, "Why not call it the ether and be done with it?"

Would Newton have called it space or something else? Kelvin once said that "If Newton were here present, in this very room, at this very moment, he would be found to be perfectly able to take care of himself". Think of what Newton said about gravi-

tation without a medium. In a letter to Bentley he remarked "That gravity should be innate, inherent, and essential to matter, so that one body can act upon another at a distance through a *vacuum*, without the mediation of anything else, by and through which their action and force may be conveyed from one to another, is to me so great an absurdity, that I believe no man who has in philosophical matters a competent faculty of thinking, can ever fall into it. Gravity must be caused by an agent acting constantly according to certain laws; but whether this agent be material or immaterial, I have left to the consideration of my readers".

The latter remark by Newton is of extraordinary interest at the present day. One thinker replies to Newton—I consider that the phenomena proceed as if space were curved: another replies—They go as if an appropriate ether exists. Both say "as if". Both use analogy. Which view might have appealed to Newton today as the most satisfying, as the least blank?

Further, the notion of a field of force did not originate with Maxwell's introduction of an electromagnetic field. Newton envisaged it for gravitation, as the above quotation shows. Still earlier, in his "De Magnete", Gilbert dealt with an "orbis virtutis".

In certain connections it is said that space, though having multiple functions to perform, is tied inseparably to time in that performance. Minkowski's development of the relationship is one of the great features of relativity theory; but the question remains, what its true relation to reality is, whether or not the so-called space-time continuum is more than a mathematical or abstract entity. While attempting to find an answer to that question, it is necessary specially to remember that all our knowledge is based upon postulate.

We may assert that our mental idea of tridimensional space is imposed upon us through our experience of three independent freedoms of motion. And we may postulate further that our spatial existence is really in four dimensions, while we have no spatial cognizance of motion in the fourth, but, on the contrary, get from it our idea of local time. Next, taking only the case of

the special theory of relativity, we may assume that the motion of a world point in the four dimensional space is one of constant speed—the speed c of light. Finally we can assume that the four dimensional space is Euclidean.

Taking, dx, dy, dz, $d\theta$ as elementary space steps in the four component directions, the resultant elementary step, dS, in four dimensions, is given by

$$(dx)^2 + (dy)^2 + (dz)^2 + (d\theta)^2 = (dS)^2.$$

Absolute time in the four dimensions, observable only by a four dimensional being, is given by the condition $cdT = dS$, where dT is the corresponding elementary step in absolute time. And, by our convention, $cd\tau = d\theta$, where $d\tau$ is the corresponding step in local time. Thus we have

$$(cd\tau)^2 = -(dx)^2 - (dy)^2 - (dz)^2 + (cdT)^2$$

which is in the form of the fundamental equation of special relativity. As in special relativity, local time agrees with absolute time for a point at rest in space. But absolute time is unobservable in tridimensional space. So, substituting t for T, where t is tridimensionally measured time, we have to express the condition which t must satisfy.

Now the time T in the four dimensional space is, by postulate, in absolutely steady flux, and the displacement $d\theta$ is in a single direction which is perpendicular to all directions in our given three dimensional space. Therefore $d\theta$, as described in any definite dT, must be invariant for any given speed of tridimensional motion. So $d\tau$ must be invariant also; and that invariance must hold for it with respect to the tridimensional quantities x, y, z, and the time t, which, from our tridimensional limitations, we register instead of the absolute time T. With this substitution of t, the above equation becomes the relativity "interval" condition.

We can now, if we adopt these considerations, apply Minkowski's procedure and so exhibit the relations of different sets of the four coördinates as due to an imaginary rotation of axes in the four fold continuum called space-time. But, on the view

above assumed, no rotation of a real time axis can take place. We cannot choose our time axis as we please if we are situated in a three dimensional universe *located* in four dimensional space. The continuum is a real four-fold space continuum, the conception of motion in which requires the idea of time as well as of space; while the arbitrary limitation of the speed of motion of each world point to the (constant) speed of light introduces the special relativity conditions. Minkowski's beautiful analysis is, if we take this view, a merely mathematical device which enables us to arrive at the results needed for transformation from one to another set of uniformly moving axes.

Einstein's freeing of the law of relativity from restriction to the case of sets of axes in uniform relative motion gives logical completeness, and unavoidably brings in the picture of curvature of space. But that space is not simply subject to coördinate measurement. It has to bear physical burdens also. We are only entitled to say that it acts as if it were curved in the neighbourhood of matter; and we have to recognise the necessity for a slight change in the expression of the law of gravitation as given by Newton, and a slight change in the refractive index of "space" from unity in the neighbourhood of matter; also an apparent repulsion of matter resulting in an expanding universe. But the question as to the reality of that expansion still remains. Acceptance of the principle of general relativity compels us to acceptance of an appearance of expansion: our spectroscopes must, in that case, give apparent corroboration. The question whether this is real remains; change of wavelength is all that is found to occur.

The whole problem is bound up with the question of the entire correctness of the dictum that investigators in physical science should confine their attention to "observables" only. But nature may contain "inferables". And omission of consideration of them as possibilities may lead merely to a *non-possumus* attitude stating that Here the possibility of further knowledge ends. It may. For example, if the presently accepted atom of action, h, be indivisible further information can only be statistical. But there may, so far as present knowledge goes, be a further dis-

covery. And, in any case, to assert absence of "causation" is to assert a mere postulate. And to talk about freewill in such a connection is worse than delusive.

One point wherein recent progress of knowledge in physics exhibits a very fundamental advance is in connection with the conservation of mass. We now know that the mass of radiation has to be included in the estimate, and we know that conservation of energy still holds. We can say "No mass, No energy". But that is no new statement in physical science. Old statements were of the type "Matter is the necessary vehicle of energy", or "Matter is energy-capacity", &c. Both statements are really exact equivalents. One statement, nearly half a century old, was to the effect that matter might disappear if energy were not present. In fact this was an essential consequence of the vortex atom theory of matter. And, in that theory, each atom was regarded as filling all space.

A new result of relativity theory is that mass depends upon speed of motion, and this in such a way that, if energy disappears, so does matter. Yet this scarcely justifies the assertion that mass and energy are identical. Individual particles of matter, individual electrons, are, in theory at least, identifiable. Individual quanta of energy are not identifiable. In fact relativity gives no *explanation* of mass and its dependence upon speed. It merely compels the acceptance of the result. It tells us that somehow or somewhy the result must occur. It tells us the mathematics of it, the physics of it is left unsettled. Mere mathematical space, room-for-scope, even with time added for motion, cannot solve the problem. The *Ifs* and *As Ifs* laid down by relativity are insufficient. We need ether, whatever its properties may be. And that brings in electrodynamics.

Conservation of energy, in its totality of forms, together with intertransformability of all its forms, ensures, if only by analogy, a dynamical theory—whensoever and howsoever it may be attainable.

Quantum theory, in the hands of Planck and Einstein, has given us transference of energy in bundles, even in the case of radiation; and has overcome the difficulties of energy distribution

throughout spectra, ionization by X-rays, photoelectricity, and so on. But it, notwithstanding enormous advances, is still unable to show why radiation in some connections seems to be propagated in wave form and in others to exist in particle form. Relativity theory, in its extreme shape, involving denial of the existence of an ether, can only accept the photon *As If* existent. The *explanation* of its existence has been given by Sir J. J. Thomson; and the satisfactory nature of that explanation is that it involves the acceptance of Maxwell's electromagnetic ether obeying just those Maxwellian laws which relativity theory accepts.

We may think of an X-ray pulse containing the energy $h\nu$, or a similar pulse originating in radioactive processes. We cannot control the emission of these pulses, but we can imagine them as occurring in regular succession so that they simulate continuous wave propagation of wavelength c/ν, where c is the speed of light. This radiation contains, per wavelength, the quantum of energy $h\nu$, due to Maxwell's "electric displacement" in the ether, which exists as energy of ether stress. Over a wavefront in which the displacement is a maximum, and whatever be the radius of the wavefront, the oppositely displaced electrifications are tending to recombine. And, if slip occurred, the energy, $h\nu$, of the corresponding pulse, would disappear out of the wave propagation. All over the wavefront the separation of the electrifications is of less order than the wavelength. So that, if slip could be started as the pulse passes over an atom, we might obtain an *As If* picture of the extraction of a quantum of energy from an extended wave train in a period small in comparison with $1/\nu$. But we can get no picture, apart from a merely mathematical one, of the nature of a blank cheque, without the medium.

It is a great step to be able to recognise how much knowledge we can attain to without the intervention of "unobservables", but a stage must arrive at which we feel compelled to use our powers of inference in order to get a satisfying illustration.

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Logic and Probability in Physics[1]

BY

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GENERAL philosophy claims to be the critical subject which lays down for all of us what we may be allowed to think, and yet it has played no part whatever in the great revolutions of human thought of the present century—those connected with relativity and the quantum theory. It might have been expected that the scientists would have been constantly consulting the philosophers as to the legitimacy of their various speculations, but nothing of the kind has happened. Since no one can dispense with some sort of metaphysic, each scientist has made one for himself, and no doubt they contain many crudities, but it would seem that a deep interest in metaphysic is a disadvantage rather than an advantage to the physicist—at least I have the impression that those of my friends who are most inclined to speculate on the ultimate things appear to be the ones whose scientific work is most hampered by doing so. Now I propose to risk a similar indiscretion. I want to embody in it the practical philosophy of a physicist, and I do not mean it as an attack on the pure philosophers, who are very reasonable people, only chargeable with the minor offence of not having made me want to read their books!

I had better begin by stating shortly the ideas I intend to discuss. There is a notable contrast between the way we think about things and the way we think we ought to think about them.

[1]Presidential address to the Mathematic Physical Section of the British Association, at the Cambridge meeting in September, 1938.

We have set up as an ideal form of reasoning the formal logic which has held the field since the days of Aristotle. We rarely conform to this ideal, but instead we usually make use of arguments having no accurate axiomatic basis, which compel belief because of some large accumulation of favourable evidence. I intend to develop the idea that the old logic was devised for a world that was thought to have hard outlines, and that, now that the new mechanics has shown that the outlines are not hard, the method of reasoning must be changed. The key to the modification has already long been in our hands in the principle of probability, but whereas in the past constant attempts were made to fit this into the old system, the new mechanics suggests the possibility of a different synthesis. Though I hope this subject will be found interesting in itself, I would not have ventured to bring it forward if I had not also a very practical purpose in doing so, and that is to urge that our mathematical education both at school and university has been gravely deficient in that it has put all the emphasis on matters susceptible of rigorous proof, while it has very completely neglected the equally important subjects of statistics and probability. I shall enter into these matters at the end of my address.

I have said that there is a contrast between the way we all think about things and the way we think we ought to think about them. This is true not merely of the scientist; the layman holds the same belief. I may exemplify this by a quotation from that epitome of the reasoned thought of the ordinary man, the detective story. After Watson has expressed admiration at one of the most brilliant guesses of Sherlock Holmes he is met with the reply: "No, no; I never guess. It is a shocking habit, destructive of the logical faculty." The reader is encouraged to revere the great detective by being told that all his arguments are Aristotelian syllogisms. The scientist forms his opinions in much the same way as Holmes really did, but he is apt to feel that this is a fault in himself and that he ought to be forming them by the severe principles of formal logic in the manner that appealed to Watson. Of course there are branches of knowledge for which this can be done, but somehow they are not the interesting ones;

indeed, outside pure mathematics any subject is apt to become dead and uninteresting as soon as it is brought down to this form. The really live branches of physics call for a very different kind of thought, for a review of a system of interconnected facts and for a perception of conjectured analogies, and so on. This is vaguer, but it is more important, and our system ought to give importance to the important things, so that the actual habit of thought which the intelligent man finds the most useful is acknowledged as the right one.

In general literature there is a particular kind of writing which we all admire on account of its direct simplicity; it is to be found in much English of the seventeenth century, but specially in the work of many French authors, both early and late. It is a delight to read and often admirably achieves its aim of clarifying the subject-matter—but not always. There are two ways of writing simply about a subject. One is to understand and make clear the simplicity of it; the other is to leave out all the difficult parts. When we entertain the idea that everything can be brought down to the Aristotelian syllogism, are we not doing this last? Is it not possible that when a subject is brought down to these terms, it is merely that we have picked out from it the easy parts and concealed all the rest? If we turn our attention to the question of why we believe in our various theories, we can see that there is often a quite illusory simplicity in their presentation.

Why do we believe in the various theories that we are all agreed to accept? Once a theory has become well established someone usually gets to work to find a system of axioms, postulates, indefinables and so on from which it may be derived. For example, classical mechanics is based on Newton's Laws, or whatever system has been substituted for them by later criticism. The direct derivation of everything from an axiomatic basis has an attractive simplicity, but it tends to make us think we believe the theory because of the axioms, whereas the axioms are only a convenient shorthand summarising a wide field of information, and they are believed in merely because we believe in the theory. This may be seen by an occurrence of a few years ago. There

was a letter to *Nature* pointing out a rather fundamental contradiction in the quantum theory—I do not think the author meant it as strongly as the accident of his wording implied. One's immediate feeling was that the idea must be wrong (as indeed it proved to be), but the point of my mentioning it is that I found I did not care much whether it was right or wrong, because the quantum theory must be right anyhow. The cumulative weight is so overwhelming that it is not conceivable that anyone could upset it in a single column of *Nature*. A little doctoring of the axioms would certainly put the matter right again, and hardly anyone would be any the better. We have then to believe that axioms are not important things, but that it is the whole body of accumulated doctrine that matters.

Take it from a different angle. The "logic" school of thought has in its repertory the idea of a "crucial experiment," that is the single experiment which gives the answer yes or no to a whole theory. I suppose the most striking crucial experiment ever done was the Michelson-Morley experiment on æther drift, which was made the basis of the whole gigantic theory of relativity. Michelson and Morley showed that to the order of the square of the earth's velocity there was no æther drift, and they showed it to the limits of the precision of their apparatus. For some twenty years the theory of relativity grew enormously, based on this one experiment, and then it was felt that it would be proper for some one else to repeat the work, and Dr. Dayton Miller undertook the task. We cannot see any reason to think that his work should be inferior to Michelson's, as he had at his disposal not only all the experience of Michelson's work, but also the very great technical improvements of the intervening period, but in fact he failed to verify the exact vanishing of the æther drift. What happened? Nobody doubted relativity. There must therefore be some unknown source of error which had upset Miller's work. But as Miller was improving on Michelson, this contains the implication that Michelson's work must have had two unknown sources of error which happened to cancel one another. What has become of the crucial experiment? We do not believe in relativity because of the Michelson-Morley experiment; it is one, and an im-

portant one, of a number of cumulative pieces of evidence which all fit together, and it is this cumulation and not any one of its pieces that makes us believe in relativity.

From examples like these we conclude that an axiomatic basis, of the kind demanded for the operations of formal logic, is too narrow for the understanding of the physical world. Something wider is needed. Now for more than a century there has been growing up the recognition that probability plays a part in much reasoning, and that there must exist a wider system of logic which has probability as one of its features. Attempts have been made, and are still being made, to bring probability back into the narrow fold of the old logic. It appears to me that these attempts are hopeless, but before approaching the question directly I want to develop an analogy which seems to me important. Like everyone else I *feel* the compelling power of the old logic, and I cannot *feel* how when we try to go beyond it we can get the same compulsive force. But on the other hand I *know* of a case where our thoughts are driven in one direction by a force which seems to have the same psychological compulsion as does formal logic, and yet where the result is undoubtedly wrong.

To anyone who has thought at all seriously about the world, or at any rate to anyone who has made an elementary study of mechanics, I suppose there is nothing more absolute than the law of causality. By this I mean that the future is completely contained in the present. Passing over obvious examples where this is true, like the path of a projectile or the orbit of a planet, we may take an extreme case where we might expect our faith in the principle would be most severely tried. Take the typical case of chanciness, the tossing of a coin. We know that in a general way there is an even chance of heads or tails, even though we sometimes hear of gifted individuals with muscles so delicately adjusted that they can control the event. But in the ordinary way the tossing of a coin is complicated by being produced by a living organism, so let us simplify the problem by designing a catapult of some kind to project it. Which of us does not believe the coin would fall the same way every time if such a mechanism could be made with really complete precision? When the

machine fails to make it do so, we say it is because there may have been a speck of dirt in the lubricant or something like that. In other words, we do not feel that the fall of the coin is determined by chance, but we regard the uncertainty we observe as due to our ignorance of all the detailed causes. Ignorance is a confession of incompetence, and so we regard the existence of chance as a blemish in our otherwise admirable characters. This feeling goes very deep, since we are prevented by it from having the complete control of our surroundings that we somehow think should be our due. We start prejudiced against probability and in favour of causality. So much for what we *feel* about causality; and about thirty years ago this feeling would have been regarded as a piece of inescapable reasoning, with the same kind of compelling power as a logical syllogism. We still have the feeling, but now we know it is wrong, and what is more, we know that it is wrong for a reason we never thought of. To understand this oversight we must review the recent history of atomic theory.

The history of the development of physics in the first quarter of the twentieth century will rank as one of the greatest in the advancement of knowledge, but it will also rank as one of the most curious in the history of human thought. In 1901 Planck started the quantum theory. Even this was curious. He was trying to find out the law of complete radiation by the use of ordinary statistical methods, and observed that he got his answer at what should have been the last stage but one of his work. The last stage would have involved proceeding to a limit, and he found that he got the experimental answer without doing so, and an absurd answer if he did. The work went rather deep into statistical theory and there were many for long afterwards who were not convinced of its compelling force, but it was the great merit of Planck that he knew that he had got something involving a quite revolutionary idea—the quantum. In succeeding years other phenomena were seen to involve the same revolutionary idea: Einstein's theory of the photo electric effect, and of the ionisation produced by X-rays, his theory of specific heats, later improved by Debye, and Bohr's theory of spectra. All these things fitted in quite obviously with the quantum, but quite as

obviously they violently contradicted the physics of the nineteenth century. What should a man think about a beam of light which according to Einstein had to be composed of arrows, whereas a hundred years earlier Fresnel had *proved* that it was a system of waves? What does a rational being do when faced with two mutually contradictory but both indubitable pieces of evidence? It was a nice test for the critical spirit, and it revealed a wide divergence of choices. In making a historical judgment long after the event, one of the hardest things to do is to recall the relative scale of importance which contemporaries were inclined to attach to the different branches of their subject.

The statistical theory of matter had already been well established by the work of Maxwell, Boltzmann and Gibbs, but it was not regarded as an essential part of a general mathematical-physical education. For example, in the various courses I was advised to undertake during my undergraduate career, no one at any stage ever suggested to me that I should learn anything about the kinetic theory of gases. I think that that period was one when the Cambridge mathematical school was not at its best, and very probably a little more was done at other places, but, to judge by the available text-books in any language, statistical theory was not regarded as one of the prime subjects of study, as it would be now. The period was essentially dynamic, and as such it was moderately easy for it to take in the new ideas of relativity, to which indeed the experimental work of the last century had been leading. But there was no common habit of thought on statistical lines, and so there was a sharp separation of opinion. The seniors, impressed with the vast mass of successful physics of the nineteenth century, with only a rather general knowledge of statistical theory but no facility of thought in it, found the new ideas completely contrary to their convictions. Such men would think that these ideas depended on the difficult and unfamiliar conceptions of statistics and would be inclined to judge that there must be a fallacy in the statistics which would be cleared up later. On the other hand the laboratory workers, dealing with atoms and electrons from day to day, could not fail to be more impressed with the discontinuous phenomena and the

beautiful way these could be explained by the quantum. Such men would cheerfully accept the Bohr orbits as a complete explanation of the hydrogen spectrum, and certainly in many cases would be actually ignorant of the difficulty, the monstrous absurdity, of supposing that a sharp jump from one orbit to another could be responsible for a train of waves shown by the spectroscope to be lasting for quite a long time. So the majority of rational beings behaved in the natural human way of managing to forget all the disagreeable facts. But not every one, for there were Bohr and the other leaders who recognised the difficulties on both sides but could still maintain an attitude of balance and could believe that from somewhere there would come a higher synthesis by which everything would be fitted together.

As time went on the quantum got obviously stronger and stronger, and began to invade more fields. The nuclear atom in the hands of Bohr showed itself capable of giving all the broad details of the periodic table of chemistry, still with nothing done to meet the awful difficulties of optical theory. But about 1925, guided by the Correspondence Principle, things were moving towards a tentative theory of the refractive index, and it was this that finally suggested the break in the contradictions. Acting on a hint given by the theory of refraction, Heisenberg was led to the suggestion that the contradictions of atomic theory would disappear if one adopted the idea of non-commutative algebra in dealing with the motions of electrons in an atom. Then the floodgates broke and the whole New Quantum Theory burst forth. It would of course be an incomplete account of it not to mention the quite different approach made independently by de Broglie and Schrödinger. If we are to trace this to its origin we must go back a century to Hamilton, for it was his work in geometrical optics which showed how a wave of short wavelength could be treated as a ray. It was de Broglie who worked out the modern analogies, but it was Schrödinger who succeeded in giving its full form, and by the invention of the *wave-function* placed in the hands of the mathematicians the most powerful of weapons for the technical discussion of atomic problems.

At first the work was of a formal kind, obviously right, and a

complete synthesis of the rival doctrines of particle and wave mechanics, but there is a very interesting point that has gradually emerged in connection with the discovery. In his first paper Heisenberg laid great stress on the idea of building theory only on directly observable quantities. It is not very clear how the distinction was drawn. The electron's orbit is certainly not observable, but is it less so than the electric force which is the amplitude in the light-wave emitted by the atom? It has seemed to me that it was not this idea of using the observable that was the merit of his work, but rather the contrary—the capacity for carrying through a formal mathematical analogy without ever asking what it all meant in terms of observable things. However that may be, it was only a year later that he remedied the defect by making a picture of his process by means of the Uncertainty Principle. I may remind you that the Uncertainty Principle asserts that it is impossible simultaneously to measure the position and velocity of any body, because the measurement of either inevitably produces a change of indeterminate amount in the other. The subject has been so often discussed that I am not going into it now, but as it concerns the centre of my argument, I want to emphasise its negative side, which as I think is much the most important. In this rôle the Uncertainty Principle is to be regarded as the argument used to defeat the old-fashioned physicist who claims that there is at any rate ideally no limit to the accuracy with which both position and velocity can be simultaneously measured. He has to admit the correctness of experiments such as the Compton effect, and we show him that by his own admission he will be defeated. On the positive side the principle is not so useful, because once we have seen the reason for the failure of classical ideas, we had better take advantage of the full technique of the quantum mechanics. Here my point is that the Uncertainty Principle showed up a fallacy in the old arguments about causality, and it was a fallacy about which we were so unconscious that we did not even know we were making it. It is now easy to see that there was nothing wrong with the old inference that if I know all about the present I can forecast the future exactly; the trouble was the impossibility of knowing the

present. Once this is seen the whole argument becomes obvious, but nobody saw it until Heisenberg. We had somehow to avoid the compulsory causality of the old mechanics, and there seemed no loophole allowing us to do so until the Uncertainty Principle. Knowing what we now know we may ask why no one discovered the loophole by applying a strict analysis, for example by the use of symbolic logic. Such an analysis would presumably have revealed the fault, but the trouble is that it would also have revealed other unwarranted assumptions which we have made but which we do not in the least want to doubt, so that it would not really have helped in pinning down the exact point of error. It is invention, not criticism, that leads to the advance of knowledge.

Following up the later history of the subject, the success of Heisenberg in exploiting the idea of observables for atoms seemed to repeat the brilliant success of Einstein twenty years earlier in using the same idea over relativity. It seemed to imply that what was wanted in physics was to free ourselves of all abstractions and only make theories about real things. There grew up a great cult of doubting the reality of unobserved things, and then a curious thing was found; the charm did not work again, and only a few minor things have come out of it. The work of the New Quantum Theory has in fact run most surprisingly in the opposite direction. The technique is largely concerned with wave-functions, which are quantities much more abstract than anything in classical mechanics. There is certainly nothing observable, or even picturable, about waves propagating themselves in many-dimensional space with abolutely unknowable phase, and with intensity controlled by the curious extraneous rule of normalisation. Largely by the use of these wave-functions the whole of atomic physics has been reduced to order, and so has molecular physics, except that it yields problems in which so many electrons are interacting that a full discussion is not feasible. So the doctrine of theorising only about observables was not really a useful doctrine; it merely provided a germinating idea. In fact we may well ask what an observable is, and if we go at all beyond direct sensations, which as physicists we certainly intend to do, the answer becomes perfectly indefinite. This opinion I heard ad-

mirably expressed a few years ago by the late Prof. Ehrenfest. It was in a physics meeting in Copenhagen and someone was proposing a way out of certain difficulties which involved, as he maintained, a reversion to the cult of the observable. Prof. Ehrenfest said: 'To believe that one can make physical theories without metaphysics and without unobservable quantities, that is one of the diseases of childhood—*das ist eine Kinderkrankheit.*'

I have dwelt at some length on the history of the quantum theory because I think it serves as an analogy to the deeper question of what is wrong with the old logical processes. Just as we used to feel the all-pervading compulsive force of causality, so we feel the all-pervading force of pure logic. Just as we felt that classical mechanics provided no room for anything beyond itself, so we feel that the old logic is the only admissible kind of reasoning. We knew that certain things led to the Old Quantum Theory and obstinately refused to fit into mechanics, and we know that the principle of probability can cover many things outside the old logic. Many men tried to force the quantum into the classical system, and many are still trying to bring probability within the fold of the old logic. I do not believe it can be done. This is not the occasion, nor have I the capacity, for a deep argument on the place of probability in logic, but one of the most convincing ways of seeing it may be found in the consideration of another branch of physical theory, the kinetic theory of gases.

In the early days of kinetic theory the central problem was the law of distribution of velocities of the molecules, and attempts were made to prove the law absolutely from dynamics, but the process always failed. Maxwell made the assumption that with the lapse of time a system of molecules would pass through all possible phases. There are technical difficulties in the discussion of this assumption which have never been overcome, and it is quite uncertain if it is even true. Indeed Kelvin, who disliked the whole kinetic theory, argued with some force that the only examples anyone could give contradicted the principle—for example, the motion of the planets. The greatest contribution to the subject was that of Gibbs, who recognised that there had to be

a big assumption somewhere and made it quite frankly and without attempt at justification. The works of Gibbs are not easy reading; in both his great works he attends to every detail with a particularity that is really rather tedious, whereas his basic ideas are thrown at the reader almost without explanation. The idea of a canonical ensemble is a really beautiful idea once you understand it, but where does it come from? An ensemble is an idea which will be unfamiliar to many, so I had better explain it. We want to know something about the behaviour of a complicated system composed of a great many parts; say we want to know the pressure of the gas in some vessel. If we tried to attack the question by pure mechanics, we should be faced with an enormous number of mechanical equations for the motions of the molecules, and even if these could be solved the solution would be of no use, because it would depend on the initial positions and velocities of the molecules, and these we should not know. Instead of trying this impossible and useless task, Gibbs considers a very large number of possible states of motion of the set of molecules, which have some character in common such as their total energy, but which are otherwise unrelated. Though each specimen of the motions is quite independent of all the others, he looks at them all together; this explains the word *ensemble*—I do not know why he had to take a French word—and makes the assumption that the pressure of the gas is correctly given by the average of all the specimens. The actual gas in the vessel at any instant is one of the specimens; in its motion it passes into configurations corresponding to others, but only after a fantastically long time would it go through even a perceptible fraction of the whole ensemble. Gibbs is assuming that the behaviour of the actual gas will be determined by the average of the uncountable millions of specimens in the ensemble. Almost at the start one finds oneself presented with the ensemble with hardly an attempt to explain where it comes from or why it is right, and the beginner is usually troubled by the fact that, though the subject is obviously mechanical, all the mechanics he laboriously learnt in his youth seem to have faded into comparative unimportance. There are various kinds of ensemble, the chief of which is the canonical, corre-

sponding to all the possible motions of the gas which would have the same temperature. Later, almost as a concession to human frailty, Gibbs introduces the micro-canonical ensemble, composed of much fewer specimens because they all have exactly the same energy. This is usually welcomed by the beginner because it seems closer to his familiar mechanics, but with more experience he will realise that the gap is still so great that he is really no better off, and he may as well accept the more general idea at once.

With the old mechanics all this involved ideas which for many readers were distinctly hard to accept. The principle of probability, embodied in the averaging over the ensemble, was frankly laid on top of the logical principles of Newtonian mechanics, and to anyone believing that probability would ultimately be brought down to the old logic the association was most repellent. But we can now see that Gibbs was a prophet far ahead of his time—and indeed, to be frank, far ahead of his own knowledge—for the new mechanics accommodates the ensemble very much more easily than did the old. The new mechanics has shown us that it is impossible to know how the individual molecules are moving, because when one undertakes an experiment to see, that experiment automatically alters the condition of the gas and so fails to tell what was wanted, the state of the molecules without the experiment. In the old days one used to feel that the validity of Gibbs's idea would be spoilt by some skilful experimenter who would really observe the motions of the individual molecules and would therefore rule out the legitimacy of averaging over the *whole* ensemble, but we now know that there is no danger of this. The real gas in the vessel is not merely one specimen of the ensemble, unrecognisable only because of our clumsiness; it is itself the whole of the ensemble. We used to think of the gas as *either* in the state A, *or* in the state B, *or* in C, but according to the new physics we have to think of it as in *all* the states A *and* B *and* C. The distinction is typical of the change we must make in our habits of thought, and most of us resist this change strongly, for we find we can hardly help asking: 'But which state was it

really in?' As I have said, we used to be ashamed of ignorance, but we must now realise that *this* ignorance is one of the things that makes the world possible. The principle of probability, which used to be loosely superposed on the old logical principle, is now with the new mechanics fully united with it in a higher synthesis.

Before leaving Gibbs I would like to refer to one thing in his book, where I think he has not even yet come into his own. He considers various types of ensemble of increasing generality. In the micro-canonical the members all have the same energy. Now we never know the exact energy of the gas in a vessel, so that a better idea is the wider one of a gas at a given temperature which therefore has a certain range of admissible energies. This is represented by Gibbs's canonical ensemble, and it is the main one that he uses. In both these the number of atoms in the ensemble is constant. But in the last chapter of his book Gibbs introduces a still wider ensemble. He calls the ones with a constant number of atoms *petits ensembles*, which I shall translate as petty ensembles, and regards them as parts of a grand ensemble in which the total number of atoms is not fixed. He uses the idea to some extent in connection with semi-permeable membranes, but on the whole does not get far with it. As in much of Gibbs's work, it is the idea itself, rather than what he does with it, that is important. This idea of the grand ensemble is not yet incorporated in the new physics. In the quantum theory we take a number of electrons and nuclei and, allowing for their interactions, we construct something that is practically the canonical ensemble. But we take fixed numbers of them—this is partly reflected in the technical process of using normalised wave-functions. Now in an experiment dealing with a large number of particles we are never really sure exactly how many there are, and to assume this number is much like assuming a constant energy for them. If the canonical ensemble is a better idea than the micro-canonical, then the grand ensemble is superior to the petty ensemble. In the new mechanics nobody has yet succeeded in making anything of it, or has made any proposal how to do so, but I will venture the

forecast that when some of our present difficulties in the quantum theory are cleared up, it will be found that we shall be using the grand ensemble with its indefinite number of atoms.

Reverting to my main theme, what is the moral of all this? It is that the new physics has definitely shown that nature has no sharp edges, and if there is a slight fuzziness inherent in absolutely all the facts of the world, then we must be wrong if we attempt to draw a picture in hard outline. In the old days it looked as if the world had hard outlines, and the old logic was the appropriate machinery for its discussion. Things went wrong when it was found necessary to call in the help of the principle of probability; this appeared first as an alien, but there was hope in the old days that the alien might be naturalised. It has resisted the process and we now recognise that it cannot be assimilated, because it provides the necessary step to a wider reason, that of the new fuzzy world of the quantum theory, a world which is not contained in the old. How far it will be possible to make a full synthesis of the new and the old I do not know, but I like to think there is something in my analogy from the history of the quantum theory, and to suppose that we are still in the condition corresponding to the Old Quantum Theory, and that some day a real synthesis will be made like that of the New Quantum Theory, so that there will be only one thing in the world that has not indefinite outlines, and that will be a new reformed principle of reasoning.

I may fitly conclude this part of my subject by returning to the point from which I started. As an example of what the ordinary man regards as correct reasoning I quoted some words of Sherlock Holmes. I must now confess that I was not quite sincere in my quotation; the impression I gave was the impression the reader carries away, but on examining the text I was interested to find that the great detective had himself arrived at the ideas I have been putting forward. In the sentence before he said 'No, no; I never guess. It is a shocking habit, destructive of the logical faculty,' he had said: 'I could only say what was the balance of probability—I did not expect to be at all accurate.' The master-mind uses the word logic in its modern sense.

There may be a feeling among some that the very general suggestions I have been making are open to every sort of criticism. Perhaps they are right; as I have said, it is part of my doctrine that the details of a physicist's philosophy do not matter much. But whether it is wrong or right, my next point is one on which I do very much hope that there may be a consensus of agreement. This is that the subject of probability ought to play an enormously greater part in our mathematical-physical education. I do not merely mean that everyone should attend a course on the subject at the university, but that it should be made to permeate the whole of the mathematical and scientific teaching not only at the university but also at school. To the best of my recollection in my own education I first met the subject of probability at about the age of 13 in connection with problems of drawing black and white balls out of bags, and my next encounter was not till the age of 23, when I read a book—I think it was on the advice of Rutherford—on the kinetic theory of gases. Things are better now, but mathematicians are still so interested in the study of rigorous proof, that all the emphasis goes against the study of probability.

Its elements should be part of a general education also, as may be illustrated by an example. Every month the Ministry of Transport publishes a report giving the number of fatal road accidents. Whenever the number goes up there is an outcry against the motorists, and whenever down, of congratulation for the increased efficiency of the police. No journalist ever seems to consider what should be the natural fluctuations of this number. A statistician answers at once that the natural fluctuation will be the square root of the total number, and apart from obvious seasonal effects that is in fact about what the accidents show; the number is roughly 500 ± 25. The proof of this does not call for any difficult mathematics, neither the error function nor even Stirling's formula, but can be done completely by the simple use of the binomial theorem. There is no mathematical difficulty that should trouble a clever boy of 15; it is only the train of thought that is unfamiliar, and it is just this unfamiliarity that is the fault of our education. The ideas and processes connected with the

inaccuracy of all physical quantities are much easier to understand than many ideas that a boy has to acquire in the course of his studies; it is only that at present they are not taught, and so when met they are found difficult.

This is not the place to describe a revised scheme of education. I would only say that it is not special new courses that are needed, but rather a change in the spirit of our old courses. When a boy learns about the weighing machine, emphasise its sensitivity, and consider the length of time that must be taken for the weighing. When he has a problem on projectiles, make him consider the zone of danger and not merely the point of fall. At a rather higher level, but still I should hope at school, introduce the idea of a distribution law; for example, in doing central orbits work out Rutherford's law of scattering. Calculate the fluctuations of density of a gas, or the groupings in time of the scintillations of α-particles. All these things ought to be examples of a familiar train of thought, and not merely a highly specialised side branch of mathematics first met at the university. It is the incorporation of probability in the other subjects on which I want to insist, but there will of course remain some higher aspects —things like least squares or significance tests—which are still to be treated in separate university courses. Even these I should hope would come to be recognised as subjects of central interest and not, as they are at present, relegated to a remote corner of specialised study.

If these reforms are carried out I shall hope that generations will grow up which have a facility that few of us at present possess in thinking about the world in the way which the quantum theory has shown to be the true one. The inaccuracies and uncertainties of the world will be recognised as one of its essential features. Inaccuracy in the world will not be associated with inaccuracy of thought, and the result will be not only a more sensible view about the things of ordinary life, but ultimately, as I hope, a fuller and better understanding of the basis of natural philosophy.

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Probability, Many-Valued Logics, and Physics

BY

HENRY MARGENAU

§1. THE RÔLE OF THEORY IN SCIENTIFIC PREDICTION

THE present paper is concerned chiefly with the problem of scientific prediction. It aims at a factual analysis of the processes leading to prediction, and ventures an appraisal, in the light of this analysis, of some modern and unconventional theories of probability and truth. But although prediction is here chosen as the central issue of discussion, I do not wish to imply that, in its usual sense, it is the only or even the dominant issue of scientific research.

The reason for this cautious, though perhaps unpopular, attitude is not far to seek. In the first place an empirical survey of the various fields of human endeavor shows that the name science is not reserved for those speculative disciplines which do enable their followers to predict. There are sciences which do not make it their business to predict. Moreover, predictive ability does not of necessity constitute a body of knowledge or a set of rules as a science. If the procedures of astrology were some day found to be of "scientific validity," i.e. were shown by their nexus with established facts to be in some sense understandable, astrology would from that day on be classed as a science; but I believe that present astrological activities, while leading (presumably) to valid predictions, would even then be referred to as pre-scientific.

The conclusion seems inescapable that science cannot be defined exhaustively as an endeavor to provide facility for prediction.

Logicians, however, may well observe that science is a denotative term given immediately in discourse, and that *every* definition of denotative terms engenders empirical exceptions. Thus, if a dog were defined as a mammal with four legs and many other characteristics, it would follow that a three-legged member of that species is not a dog. The preceding remarks are therefore reduced to the simple assertion: The term science does not coincide empirically with the criticised description of it in terms of predictive facilities. But the point to be noted is that it differs widely from it. The pragmatist is then still free to assert in a normative sense that we ought to call sciences those fields of activity which allow prediction though this is in many cases contradictory to convention.—To this we answer that the norm is likely to be rejected by most people actively engaged in making scientific predictions, for their concern is far more with the features of induction and analysis which precede prediction than with prediction itself. The discovery in any field of a new "principle" is heralded as a success far greater than the extent of the future it may possibly illuminate. The discovery may well illuminate the past and be no less important.

When we speak here of prediction we do not mean the word in its temporal sense. Every complete scientific procedure involves two sets of facts, facts which are ordinarily reduced to, or phrased as, observations. Between the two sets it formulates a sort of linkage, sometimes directly and very often indirectly, in such a way that when one set is known the other may be inferred. This inference is not always reciprocal nor transitive.[1] It may involve a theory replete with symbolic constructs, or a theory of a more intuitive and simple kind, or none at all. An example will make the situation clear.

Suppose a physicist makes measurements of pressure (P) and volume (V) on a given concrete body of gas. He finds an approx-

[1] The reader is asked to pardon the vagueness of the terms. The matter is discussed more adequately and fully in my papers on Methodology, Phil. of Science, 2, 49; 2, 164, 1935.

imate relation between different observations on P and different observations on V, a relation well known as Boyle's law: $P V = k_1$. Trusting now to the principle of consistency or uniformity of nature, upon which are based certain rules of induction, he will soon be convinced of the validity of the relation and apply it to make inferences from a given or known P to an unknown V. By scientific prediction we mean this *inference* from P to V. It is important to note that no *theory*, as the word is understood in physics, is necessary to perform this passage. Required are only the operational rules leading to the numerical determination of P and V on the one hand, and a bit of arithmetic on the other.

But the physicist's interest transcends the behavior of the particular body of gas with which he has been experimenting. He will try another and arrive at the approximate relation $PV = k_2$. The new problem which now arises is to correlate k_1 and k_2. At this point the rôle of theory begins. An association is made between the operational P and a certain *construct*, the thermodynamical pressure which is designated by the same symbol P. The fact that the same letter is universally used for the two entities has blinded many scientists to the recognition that they are *logically distinct*. An easy way to see this difference is to watch what is being done with the symbol P in theoretical thermodynamics and then attempt to identify P with observational manometer readings. If this fails to convince, we may point to the fact that the thermodynamical construct P is altogether different from the construct P employed in statistical mechanics. Gibbs' formalism in particular, in which P is related to abstract properties of an abstract ensemble, provides a very forceful reminder of the logical distinction here drawn and of its inevitability. It is thus clear in the first place that the concept of pressure used in thermodynamics is different from that used in statistical mechanics. But if this is admitted it follows formally that at least one of them must differ in some way from the operational pressure, and a little further reflection will show that both, thermodynamical pressure and statistical pressure are concepts not to be identified with the operational P. One striking difference lies in the fact that, as a variable, the latter is capable of taking on

discrete values only, whereas the former two vary continuously. In the present paper we shall follow the terminology previously proposed and refer to the former two concepts as (symbolic) constructs and to the latter as a "perceptible fact" or, more crudely, as "part of nature."[2]

Between perceptible facts and corresponding constructs there exist rules of correspondence of a very subtle kind, so subtle indeed that they are frequently taken for identities. For the physicist these rules amount for the most part to habits of thought; he knows instinctively, after years of training, how to correlate the result of his measurements on P and V, with the thermodynamic or statistical notions P and V which function in his equations. The important property of these rules from the present point of view is that they are *unique*, provided the set of constructs to be used is specified. Thus from P (observational) we may pass either to P (thermodynamic) or to P (statistical) or, if there were other theories, to other constructs. The return trip is without alternatives: it must always land at P (observational).

An "explanation" of the constants k_1 and k_2 of the example under consideration involves a complete circuit of scientific inquiry. Starting with the observational relation $PV = k_1$ which is valid for a given experimental object, the physicist makes the transition to the field of constructs, using (almost unwittingly) the above mentioned rules of correspondence. Thus he associates with his object a great number of abstract properties which he is capable of ascribing also to a specifiable class of other bodies,

[2] I am fully aware that the distinction just made meets with widespread disapproval among logical positivists and physicists. To clarify my position I may perhaps be permitted to say that I admire the successes achieved by the syntactic analysis of the Vienna circle in clarifying the calculus of propositions. But I am forced to regard their fundamental proposals as an interesting "Philosophie des Als-Ob" which proceeds on the tentative rule that we ignore all "pseudo problems." This very statement implies that there must be a deeper sense in which the problems, arbitrarily ignored, still persist. Positivistic ignoration is a useful artifice indeed for it promises to solve the remaining problems with greater ease. However, methodology of science, unlike some parts of science itself, automatically breaks across positivistic landmarks, as the present distinction shows. In deference to the merits of positivistic procedures I should like to call propositions whose meaning is not clear in the formal idiom (Cf. Carnap) *forbidden* propositions, distinguishing them from "meaningless" ones which may be defined in other ways.

properties which are not necessarily themselves observable but among which there are some which allow passage, by unique rules of correspondence, to observable facts. Among these properties are the constructs P and V. He now finds himself in the field of symbolic construction in which his activities are not limited by experimental errors, least counts, practical feasibility, etc., but in which he is free to operate according to deductive procedures (embodied in his hypotheses) which he himself invents. The only requirement with respect to these procedures is that they be invariable and free from internal contradiction in the sense of the postulates of logic. These procedures plus the constructs they contain are either physical theories or idle speculations according to whether they lead, at *every* step, to situations amongst the constructs which allow transitions, by the rules of correspondence, to observable physical facts, or not. In the present instance the constructs introduced include the temperature T and, if we refer to the statistical theory, the number of molecules, their momenta, etc. By well known reasoning in which the constructs are treated like continuous numerical variables (this is one of the components of the deductive procedure peculiar to the theory under discussion; it is not common to all theories; wide quantum mechanics where symbols are treated as operators!) we arrive at the relation $PV = RT$. From this the transition is easily made to the operational counterparts of the constructs appearing in the equation, and the landing is safely accomplished. Whether the theory is right or wrong depends of course on whether, by starting with $PV = k_1$, we are led correctly to the constant k_2 for some other body.

In the case of a single body we were able to infer V from P, obviously without intervention of theory. But theory was employed in order to permit inference of V from P for an arbitrary experimental object. The sketch of the function of theory in this instance was in close alignment with the actual procedure of working physicists; it was not intended to assert that only theory could achieve the inference for an arbitrary body. To this question we return in a moment. We have established one fundamental thesis: the physicist and, presumably, every scientist,

infers one set of observable facts from another set of observable facts. The inference may be accomplished with or without the use of theory. If without, the inquiry stays in every phase of the inference within what has been called nature; if with the aid of theory, the inference leads from nature into a field of symbolic construction and back to nature. This inference, regardless of the two alternate modes in which it may be conducted, will be called scientific prediction. I believe that most scientists who employ this word adopt tacitly the same definition.

In the example chosen above, could the use of theory be avoided? In principle this is possible indeed. For there is nothing to prevent the physicist from setting up a great number of relations between observed P's and V's for different gases under different conditions, and the experimentation could be carried on without regard for hypotheses until an approximate empirical equation between all the constants k_1 is found. In the course of this process T would be introduced as an observed number, and the ultimate result, approached as a limit but never reached, would be a relation of the form $f(P, V, T) = 0$, where P, V, T stand for measured numbers regarding which no assumption of continuity need be made. In one sense, the procedure differs only by the inclusion of a third variable from that leading to Boyle's law for a given body at a constant temperature: $f(P, V) = 0$, but in another sense it differs very widely: for the experimental problem of finding a relation among 3 variables is very much more complex and more difficult than among 2.

It is quite possible that theories first arose because of the peculiar and astounding enlargement of difficulties as one proceeds in the sorting of observations from two principles of division to three or more. If nature presented us with nothing but diadic relations the necessity for theories might never have been felt. It is useless, however, to decry the value of theories in science for that reason, for it simply happens that nature is not constituted in that way but in a manner which does impart relevance to theories, at least in a considerable number of aspects. It is idle to say that prediction of facts is the only important goal of science and that theories are of ancillary service in achieving this

end, because the striking and a priori unpredictable usefulness of theory in ordering and assorting observations is to the open-minded just as significant a feature of reality as the ordered facts. If we couple these points with the undisputable circumstance that the class of scientists displays an intense interest in successful theory, all grounds for the pragmatic assertion of the all-importance of observation in the fields of endeavor called sciences are removed.—Later, in studying the problem of empirical correlation, we shall discover further justification for the claim that theory i.e. symbolic construction, be an integral part of scientific analysis. At present we shift our attention to the two endpoints of the circuit of prediction: the two sets of observation which it connects, losing sight for the moment of whether a theory is operative in the process or not.

§2. CORRELATION VERSUS SYMBOLIC EXPLANATION

The set of observations from which prediction starts, being the result of measurements, is a group of definite numbers. The same is true of the set by which the prediction is verified or refuted. Thus arises the question: If a prediction is to be valid, must it agree with the measured set absolutely, in the manner of point coincidence? All practical experience demands the answer no. We arrive at the problem of how to soften the rigidity of prediction without destroying its essence. One thing is certain: whatever procedure we adopt to diffuse one set of observations must, by symmetry, be applied to the other as well, for the process of prediction may in special cases be reciprocal. In science this diffusion is accomplished by two well known methods: (1) the method of statistical correlation; (2) the method of introducing errors of observation. As the following analysis will show, the second method may be regarded as a limiting case of the first.

Prediction is more than the simple act of inferring one set of data from another set. Since it makes use of one of the two methods mentioned it takes on the complications entailed by them. In order to apply either method it is necessary to generate a *series* of sets of data at both ends of the predictive chain. Thus, to revert to our example, prediction passes from a single P to a

single V (the set here reduces to a single number; we might also have linked P, V at one temperature with P V at another for the same gas, in which case the set would have consisted of two numbers), whereas correlation is meaningless unless we assume a series of P's as well as a series of V's. The generation of sequences of this sort and the assumptions to be made about them form an important aspect of the problem of scientific induction, which, however, will not be dealt with in extenso here.

The methods of correlation and of errors are relevant to the main issue of this paper and will therefore be subjected to closer scrutiny. We start with the former. In a recent issue of this JOURNAL,[3] C. H. Prescott has given an interesting survey of numerous features of the problem of correlation, and we desire to refer the reader to his treatment for some parts of the subject which we can not treat adequately in this report. We state, however, that we are not at all in accord with his thesis that the ultimate domination of nature will be achieved by establishing extensive correlations, as may be seen from the remarks in the preceding section. Rather, we view the establishment of correlations as a procedure incidental to the success of science in many fields, but subordinate to the explanatory domination of nature in terms of the symbolic constructs introduced earlier. We also point out that the rules mentioned by Prescott for determining degrees of correlation, while ideally sound, are not those commonly employed by working scientists.

Let us discuss the matter in its simplest form, restricting our attention to cases involving two variables, such as P and V measured for a given body at a constant temperature. Let us call these variables x and y. To obtain the necessary series, numerous simultaneous observations, x_i and y_i, are made. Statisticians express the degree of correlation between the observations on x and those on y by means of certain numbers called correlation coefficients. Numerous recipes for obtaining such numbers are on the market, all based on sound mathematical reasoning, but measuring correlation on different scales. Clearly,

[3] C. H. Prescott, Jr. Phil. of Science, 5, 237, 1938.

the suitability of the scale depends on the problem to be treated, which accounts for the fact that actuaries may use different coefficients for their correlations than biologists do. Only one of the various possibilities will here be chosen for illustration. It has, to be sure, several disadvantages, in particular that of yielding a value equal to unity for the correlation coefficient only if the law combining the variables is a linear one. This, however, will be no source of difficulty for the present purposes.

The rule for finding the number in question, r, is this.[4] List all observations, x_i and y_i, in pairs, and let $\bar{x}$ and $\bar{y}$ be their mean values. If i runs from 1 to n, we define

$$r = \frac{\sum_{i=1}^{n} (x_i - \bar{x})(y_i - \bar{y})}{\left[\sum_{i=1}^{n} (x_i - \bar{x})^2 \cdot \sum_{i=1}^{n} (y_i - \bar{y})^2\right]^{\frac{1}{2}}}. \qquad (1)$$

One can see immediately what the limiting values of r will be. For let us suppose, in the first place, that the various x_i and y_i are entirely "independent." This means that the sum $\sum_{i=1}^{n} x_i y_i$ approaches to value $n\bar{x}\bar{y}$. In that case the numerator of (1) is zero while the denominator (barring the case of zero dispersion which is uninteresting) remains finite. Hence, for statistical ("stochastic") independence, $r = 0$. Next, let us take the case in which there exists a linear relationship between x and y, for instance $y_i = ax_i + b$. Then $y_i - \bar{y} = a(x_i - \bar{x})$, so that numerator and denominator of (1) become equal. Therefore, if x_i and y_i are linearly related, $r = 1$. (If a were negative, r would be -1.) The absolute value of the correlation coefficient, $|r|$, is confined between 0 and 1, 0 indicating *no* correlation (stochastic independence), 1 indicating exact functional (in our case linear) relationship.

But why was an expression of the type (1) seized upon to represent r? The only way to validate this or similar choices is to go back to certain axioms of probability theory. They may be summarized by saying that the series of measurements must be

[4] Cf, for instance, R. v. Mises, Wahrscheinlichkeitsrechnung.

assumed to be a probability aggregate (Von Mises' "Kollectiv", or something equivalent to it in other theories), and this involves a drastic element of metaphysics. To avoid it seems to me impossible; for even if we were willing to regard (1) merely as a conventional formula which happens to have proved helpful in practice, and by which we agree to judge the relatedness of our observations, the use which is made of r in scientific work, and the conclusions drawn from it, would be completely unjustified. If the fundamental axioms in question are not retained, the value of r has no bearing even upon the problem of prediction which has initially led us to investigate this matter of correlations. These axioms are definitely *metaphysical* in the sense that observation does not dictate their adoption. It is a rather amusing commentary on the efforts of those scientists and philosophers who wish to banish symbolic constructs from science, and who therefore talk of nothing but correlated facts for fear of permitting entrance to metaphysics, that one look behind their screen of correlations shows metaphysics galore.

The axioms referred to may be stated in several different ways. The clearest is probably the formulation of v. Mises in terms of two component postulates: Existence of the limit of relative frequencies, and "Regellosigkeit." It is true that there are difficulties connected with this formulation, especially the contradictory character of the limit postulate if the term limit is used in its mathematical sense, and the non-constructibility of the series in view of its randomness. Whether these may be resolved or not is a question affecting the validity of this specific scheme but not the fact that it necessarily transcends observations. An interesting alternative formulation which has not yet enjoyed wide-spread study among philosophers and physicists is that of Kolmogeroff[5] who treats the subject of probability and correlations as a branch of the theory of set functions, thus introducing the axioms of completely additive sets. We point out that correlations between *observations*, to which this treatment and, as we hope to show, all scientific uses of probability notions, are restricted, can be founded entirely upon a *frequency* interpretation

[5] A. Kolmogeroff, Grundbegriffe der Wahrscheinlichkeitsrechnung.
H. Cramér, Random Variables and Probability Distributions.

of probabilities and involves no concept so problematic as the probability of a single event. For a review of the various probability theories a paper by Bures[6] may be consulted.

It will be instructive to apply formula (1) to a few simple instances. As our first illustration we consider the correlation between the second digits (x_i) of the numbers and the second digits y_i of their squares, the numbers running from 11 to 30. The reader can easily make a table from which the results here given may be verified. In the first column of this table are written the numbers from 11 to 30; in the second the x_i which run from 1 to 9, then from 0 to 9, the final entry being a 0, obviously. The third column contains the y_i: 2,4,6,9,2 etc. From these one finds at once: $\bar{x} = 4.5$, $\bar{y} = 4.4$. In the next two columns are written x-4.5 and y-4.4. This shows in detail the customary procedure. One thus obtains for the numerator of (1) the number 57.6, for the denominator $\sqrt{570 \times 540} \doteq 555$. Hence $r = 0.104$. This result would be regarded as indicating a very low correlation, almost as an absence of relatedness between the x_i and the y_i.

Next, we consider the correlation between the volume of water carried in the rivers of a certain district in Sweden per year, (x_i), and the yearly amount of precipitation (y_i) in that area.[7] The procedure outlined leads in this case to the value $r = 0.705$, indicating quite definitely the presence of some "connection."

Finally, we will take a typical series of fairly accurate measurements on Boyle's law; the series is listed below for the reader's inspection (in arbitrary units).

P_i:	5.69	2.94	8.64	12.83	4.80	6.01	9.10	7.42	10.29	3.88	5.30
$\frac{1}{V_i}$:	10.82	5.70	16.40	24.39	9.08	11.44	17.28	14.12	19.60	7.37	10.11

If the value of r is computed for these data, it turns out to be 0.9999918.

Now there is absolutely no a priori cause for suspecting that, because in the last example chosen the correlation coefficient is

[6] C. E. Bures, Phil. of Science, 5, 1, 1938.

[7] This and many similar examples may be found in C.V.L. Charlier, Vórlesungen über die Grundzüge der mathematischen Statistik.

so much nearer unity than in the others, the relationship between the data expressed is of a different *kind.* So far as the observations and the method of procedure are concerned, no difference at all is present. But this circumstance must not predispose the philosopher of science against the clear realization that, in encountering cases of observations like the one in the last example, the human mind has been struck by the closeness of the correlation, by the frequency of occurrence of these cases, and that it has chosen in regard to them a new type of analysis, namely that in terms of what is called a theory. Our concern is not with the question whether theories are unavoidable—we have already emphasized that nature can in principle be completely explored by phenomenologic correlations—but with their methodological analysis when they are present. The attitude which views them as unimportant by-products of an essentially factual description of the universe is nothing but an affected naivety, a grotesque *nihil mirari* which as it were shrugs its shoulders at the sight of new and astonishing types of growth, casually suggesting that science is not concerned with them, but only with the fertility of the soil that produced them.

The transition from correlational to symbolic or theoretic procedure is made, as was indicated before, by definitely leaving the field of nature and later returning to it. This is not an arbitrary undertaking, permissible whenever the investigator pleases to indulge in it. The criterion for its permissibility involves the notion of experimental *error.* Regarding this, it is only necessary to say at present that there are well known and perfectly standardized methods for determining the experimental error of a series of observations. When this has been ascertained, the following criterion has meaning: If the defect of correlation $(1 - r)$ is smaller than a certain measure[8] of the experimental

[8] This phrase is being used to avoid fixing the exact meaning of experimental error, which may be taken to be the mean deviation from the mean, the standard deviation for measurements under "identical" conditions, or any other index used by scientists for such purposes. The following relation is illuminating in this connection: If we define

$$y_i = a(x_i + \Delta_i);\ S(x) = \sum_i (x_i - \bar{x})^2;\ M = \sum_i (x_i - \bar{x})\ (\Delta_i - \bar{\Delta});$$

error, the scientist takes or may take the liberty of abandoning the correlational procedure and to invoke the method of symbolic explanation in terms of theory. Although I know of no place where this rule is stated, I am quite certain that almost all scientists adhere to it.

Both correlation and symbolic explanation have been alive throughout the history of human thought. Ancient association between the ascent of stars and the seasons was a primitive form of correlation, mythology an early attempt at symbolic explanation in a crude way. Aristotle's endeavors were chiefly correlational, while the atomists were clearly proponents of symbolic explanation. This example is particularly interesting because Aristotle was *aware* of the fundamental distinction between the two modes of analysis, for he saw the incompatibility which existed between his philosophy and that of the atomists. At least in this respect was his thought clearer, I believe, than that of the modern pragmatist or positivist who accuses him of having retarded the growth of science. At the present time, sciences may be divided into two classes. In one, correlation of data predominates, in the other the method of symbolic explanation. Physics and chemistry belong to the latter class, descriptive biology and statistical economic science to the former. Most success has been achieved in the fields depending on symbolic explanation, and there seems to be a general feeling, expressed forcefully by the groping efforts seeking to introduce physical methods of analysis into the realm of human behavior, that symbolic explanation is a superior tool of research.—The assertion here implied of a methodological similarity between mythology and modern physical analysis may not be palatable to physicists. Yet it must be evident to the unbiased thinker that the electron and the ancient gods have very much in common as regards lack

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then formula (1) may be seen to be identical with

$$r = \left[1 + \frac{S(x)S(\Delta) - M^2}{(S(x) + M)^2}\right]^{-\frac{1}{2}}.$$

If $M \ll S(x)$ and $S(\Delta) \ll S(x)$, $1 - r \doteq \frac{1}{2}\frac{S(\Delta)}{S(x)}$.

of direct observability, their function as foci from which flow numerous observable phenomena, and as ultimate termini of inquiry. The reason, however, why mythology is not a science is found in a rather simple but significant detail: The deductive use of the constructs is neither invariable nor free from internal contradiction. (Cf. §1) Unbridled practice of symbolic explanation (speculation) does not make a field a science.

After these lengthy excursions, let us return to the problem of prediction, distinguishing now case a) (pure phenomenologic correlation) from case b) (symbolic explanation). When a correlation coefficient r is known, a set of observations to which the coefficient relates may be predicted with a certain probability p, p being a function of r. Thus in case a) prediction passes directly from one set of observations to another, the relation between the two sets involving probabilities. In case b) a theory is interposed. This theory links a sharp set of observations with another sharp set of observations; the theory itself has no diffusing effect; its elements are as clear and sharp as *logical* constructs, and its usefulness depends on this clarity. But diffusion is introduced in case b) by admitting experimental errors in both sets of observations, so that the net effect of prediction in case b) is the same as in case a): one set of data is linked by probabilities with another set.

§3. THE "PROBABILITY OF THEORIES"

The stage has now been set for a discussion of one of the most interesting problems of the philosophy of science, the problem of scientific induction. This, however, would lead us more deeply into epistemology than I feel competent to go. The remainder of the paper is negative in its constructive scope, inasmuch as it attempts to analyze in what respects the premises of certain proposed solutions of the problem of induction fail to correspond to the foregoing conditions. One of the most noteworthy recent proposals of the sort in question is embodied in Reichenbach's well known book Wahrscheinlichkeitslehre[9] and in other interesting writings by the same author. The book represents a far

[9] H. Reichenbach, Leiden, 1935.

reaching attempt of clarifying and solving age-old problems by rearing them on the basis of a new probability interpretation, and by endowing the probability concept with a universality of application which, in our opinion, it does not possess. The merit of the work in question as a speculative approach to the problems of philosophy is beyond doubt, and unaffected indeed by the present criticism; but the claim of its author[10] that his system is part and parcel of present investigations in the strict sciences, particularly in physics, must meet with emphatic rejection. Its success can not be regarded as assured in view of current scientific procedures; it can only be granted if it modifies and improves them in the future.

Reichenbach's assertion that the probability implication applies generally to the relation between theory and facts is incompatible with physical methodology, and it is at the same time the thesis which lands him ultimately in the midst of many-valued logics. If, therefore, it is possible to refute it, most of the strangeness of his system—and perhaps its uniqueness—disappears. Let us, therefore, concentrate on the probability of theories. It is true, of course, that physicists occasionally, though rarely, speak of the probability of a theory. But this is hardly sufficient cause for widening the meaning of the term in an elaborate way, no more so certainly than there is occasion for extending physical theories because philosophers occasionally indulge in a loose use of technical scientific terms. Yet, as was already pointed out by Nagel,[11] they never attach significance to such statements. Let us rather see what physicists actually do when they apply their theories. From Reichenbach's point of view hypotheses have probabilities even before they are linked with facts. Since his definition of probabilities is of the frequency type, the hypothesis in question must form a member of a certain class or aggregate of hypotheses in which successful ones must be distinguishable from failures. This leads, in Reichenbach's own terminology, to a probability of the "second form." The details of the manner in which he proposes to count off relative frequencies among comparison

[10] H. Reichenbach, Phil. of Science, 5, 21, 1938.
[11] E. Nagel, Mind, 45, 501, 1936.

theories and of their selection, are not of great interest in this connection; astounding perhaps is only the fact that such procedure is actually deemed descriptive of what physicists are doing. But aside from our inability of finding a single instance of it in physics, we do not see how it could possibly work if it were tried, and this for the following simple reason.

Reichenbach's own endeavor is to produce probabilities which, in the face of a given body of evidence, are unique and measurable. To achieve this result a frequency theory is needed. Unfortunately, however, even a frequency theory will fail to give unique probabilities if the elements to which it is applied are not clearly defined as single entities. A theory cannot be defined quantitatively as an entity. As an example, take the quantum theory. From one point of view it is certainly a single theory, but every physicist knows that Schrödinger's approach is in every respect different from that of Heisenberg, although the two may now be shown to be isomorphic. Is the quantum theory one entity, or does it amount to two? Moreover, it is perfectly easy to break up each of the components into a number of other theories again. This property of resolvability is peculiar to all physical theories inasmuch as every theory permits an infinite number of singular statements. The trouble here is not that the use of the term theory *has* not been standardized, but that it *cannot* be standardized. In view of this the counting of theories is meaningless; one count is as good as any other, and probabilities may be made —within due limits—anything the counter pleases.

Next we pass to Reichenbach's "probability of the first form" for theories.[12] This concerns the truth frequency of theories and is to be determined in the following way. One lists all the testable propositions to which a theory gives rise, and then divides the number of those which are experimentally verified by the total number. Now it is evident that this prescription is entirely without value when the number of testable propositions is infinite. This, as we have already seen, is always the case. Since the number of tested and hence that of verified propositions is finite,

[12] H. Reichenbach, Erkenntnis, 1935, p. 267.

the "probability of first form" is zero for every theory. This criticism is so simple and obvious that we feel the prescription, given by Reichenbach in this form, must have been intended differently. Indeed the example chosen by him for illustration suggests that the relative frequency is to be taken not with respect to all *testable*, but to all *tested* propositions. This example concerns the probability of the quantum theory and is very illuminating. (Cf. last reference, where the symbols we shall now use are defined.) ψ is the quantum theory, φ_i is a class of tested propositions having a truth-frequency $W(\varphi_i)$. Then, according to Reichenbach,

$$W(\psi) = \prod_{i=1}^{n} W(\varphi_i).$$

Now it was already pointed out that no theoretical proposition (prediction) can ever agree exactly with experiment; hence $W(\varphi_i) < 1$. The number of tested classes of propositions is n. We are thus immediately led to the result

$$\lim_{n \to \infty} W(\psi) = 0$$

In words: the probability of any theory may be made smaller and smaller by testing it long enough.

But this is not the only difficulty. It is in fact easy to show that, according to Reichenbach's definition, the probability of the quantum theory is zero at present. Quantum theory requires a certain intensity distribution in the continuous spectrum beyond the limit of, say, the Lyman series in hydrogen. This intensity distribution has been photographed. In this instance, then, theory produces an infinite number of propositions of the type $\varphi_{1\lambda}$ where λ is a continuous variable, any value of which may be tested. $\varphi_{1\lambda}$ means: At the wave length λ there is to be found an intensity of such and such a value. Now the reduced photometer trace agrees with predictions only at a few points, disagrees everywhere else. This makes $W(\varphi_1) = 0$ and therefore $W(\psi) = 0$. —It is idle to object that we must not require exact agreement because the theory predicts only with a certain probability; this

would mean putting the cart before the horse, since the present procedure is supposed to lead to a determination of that probability. The physicist knows, of course, where the trouble lies. His observations themselves are subject to error in an *empirically determinable* way. This leaves his theories sharp and connections can readily be established.

The present criticism to Reichenbach's prescription for finding the probability of a theory (of the first form) is not *in general* valid if, in carrying out his procedure, we operate with experimental errors. When the "softening" of data by means of experimental uncertainties is permitted, however, it is extremely difficult to see what methodological advantage could be gained by softening up theories as well. True, there is no rule against it, but Occam's razor has thus far prevented physicists from practicing such unnecessary diffusion.

There is a rather deep seated difference between Reichenbach's conception of a physical theory and that which we are advocating as representing more truly the physicist's view. According to the former a theory is the logical product of proposition sequences, e.g. in the case of the quantum theory $\psi = \varphi_1, \varphi_2, \varphi_3 \cdots \varphi_r$. This definition is untenable. In the first place, if the product contains a finite number of factors referring to tested sequences, the definition is certainly wrong, for every theory implies much that is not tested. In the second place, if the factors stand for all possible sequences, the logical product is an infinite one and therefore very troublesome if not meaningless. The main inadequacy of Reichenbach's conception—and this criticism may be made of the entire Vienna circle as well—is that it fails to recognize the inherent power of every theory of *generating* testable propositions ad libitum. To this most significant circumstance justice can only be done by formulating the meaning of theories in terms of symbolic construction in the way we have attempted.

§4. WHEN IS A THEORY TRUE?

In his reply to Nagel's competent critical review[13] of his Wahrscheinlichkeitslehre, Reichenbach further elaborates his views on

[13] E. Nagel, Mind 45, 501, 1936.

the rôle of physical theories in a manner most vulnerable to criticism by physicists. We shall again use his own example to clarify the fundamental difference between his point of view and that which is in our opinion almost universally adopted by working scientists. The characteristic feature of Reichenbach's thesis is that theories make predictions *only* with a certain tolerance of error. There appears to be no close relation between his "probability of a theory" discussed in the last section, and this tolerance, as indeed one frequently misses the exact link connecting Reichenbach's various pronouncements with regard to the function of theories. But they have in common the implication of a certain internal diffuseness which ultimately leads to many-valued logic. The example to be considered is the theory of the constancy of the speed of light in free space in inertial systems. This theory, Reichenbach asserts, in essence is not a unique affair, but resolvable into an infinite number of statements of the following sort: c is constant within an accuracy $\pm \Delta$ with a probability greater than p. p varies presumably from o to c, and p is a function of Δ. Thus if $\Delta = .0015\%$, $p = 2/3$; if $\Delta = .0052\%$, $p = .9999$. If one looks into the computation of these figures one finds that the partial probabilities p of the constancy theory are obtained by the rules for treating the divergence of observations in a given set experiments, such as those of Michelson and Morley, and that they have little if anything to do with the probabilities of theories of either the first or the second form as previously defined. However we are here concerned with more than the consistency of Reichenbach's own procedures; we wish to exhibit their contradiction with well established physical methods.

When the physicist speaks of the constancy of the velocity of light he means this in a far more absolute sense. His theory is either true or false; a single experimental contradiction (and this has often been pointed out before) invalidates it completely. But by contradiction is not meant a divergence from prediction, however slight. The degree of permitted divergence has nothing at all to do with the theory and is by no means arbitrary; it is derivable by an analysis of observations. Suppose that there exist a number of "good" measurements of c in different inertial

systems (the qualifying adjective is clear to physicists; it bars measurements containing discernible systematic errors), and let c_{1i} be a sequence of measured values in one system, c_{2i} in another. Finally, denote by $\bar{c}_1$ and $\bar{c}_2$ their arithmetic mean values and by Δc_1 and Δc_2 their standard deviations. The theory is then said to be true if the ranges $\bar{c}_1 \pm a\Delta c_1$ and $\bar{c}_2 \pm a\Delta c_2$ overlap, false otherwise. If *any* new series of measurements produces a range $\bar{c}_n \pm a\Delta c_n$ which does not overlap all others, the theory is declared false and abandoned. The constant a is of order of magnitude unity and is inserted as a concession to a convention which has not completely crystallized. It is furthermore supposed that every set of measurements is sufficiently large to be susceptible to statistical treatment.

The "justification" of this criterion may be questioned from several philosophic points of view. If, for instance, a theory were regarded as part of nature's permanent and transcendental constitution one might well ask whether a set of observations, judged sufficiently large to be a fair sample, may not be a statistical freak and thus accidentally fail to satisfy the criterion just formulated. The physicist's answer is simply that he is not concerned with any theory independent of his observations and that in case of such failure the unfortunate theory falls by the board, accidentally or not.

The important point is that lack of agreement with facts never reduces the probability of a theory, but causes the scientist to abandon it. Striking indeed is the readiness with which physical theories are abandoned or modified. It may even be said that the clean truth-false alternative peculiar to the validity of theories is the main spring of physical progress. For if a contradiction merely lessened their probability the incentive to try modifications would not be great, and it is precisely the constant temptation to adjust theories to facts which is responsible for the rapid development of modern physical research. *Theories* are subject to change, and not their probabilities.

A case in point is the recent discovery of the heavy electron. Had Street, Stevenson, and Anderson applied consistently the method of Reichenbach, their findings would merely have reduced

the probability that the electron's rest mass is 9.115×10^{-28} gm. Instead, they concluded essentially a failure of the theory which attributes equal masses to all electrons, and suggested a new construct, the heavy electron.

At first sight, the argument maintaining the fixity of a theory's propositions might appear indefensible in view of such specimens as the statistical theory of gases or quantum mechanics, which do operate with probabilities. Under certain conditions (non-commutability of observables) quantum mechanics allows only inferences of the type invisaged by Reichenbach in connection with the constancy of c, although with one important difference. Quantum mechanics predicts all sorts of curious probability distributions for the observed data, not only Gaussian ones, and in that respect goes far beyond Reichenbach's interpretation. If this situation were to be described in his manner, it would lead at once to probabilities of higher order; i.e. the theory would have to be construed as follows: Theory says that the probability of finding an electron at a given place is p_1, and the probability that this statement be true is p_2. This, again, is not in conformity with the physicist's interpretation, which is much the same as in the former case and permits only the values 0 and 1 for p_2. It seems that the order of Reichenbach's probability is always higher by one than the physicist's.—One cannot say that quantum mechanics proves Reichenbach's point, because this theory definitely permits conditions of perfectly sharp inference, in fact it does so in most of the interesting experimental cases (sharp spectral lines, etc.). In earlier papers[14] I have treated these matters at greater length. For the present it suffices to state that even theories, such as quantum mechanics, whose sharp inferences (ψ functions) are related to probability distributions on the side of nature in a somewhat complicated way, are always found either true or false by the criterion discussed.

§5. MANY-VALUED LOGICS

After the critical remarks of the preceding section we hasten to record again that they were not intended to damage the

[14] H. Margenau, Phil. of Science, 1, 133, (1934); 4, 337, (1937).

analytic structure of Reichenbach's probability theory, which, I feel, would be improved if the notion of probability of theories were dropped. Very interesting and fruitful is his replacement of v. Mises' condition of randomness by normalness, which renders probability aggregates constructible. His approach from the side of symbolic logic is illuminating and the formalism suggestive, although in places perhaps unnecessarily complex. Most noteworthy is Reichenbach's attempt of getting around the limit difficulty by appealing to probabilities of higher order. On this matter the last word has of course not yet been spoken, and one hopes its author himself will return to it with further clarifying thoughts. But all these matters stand even if those features which contradict scientific practice are dropped. Why, then, were they inserted?

We venture to suggest a two-fold answer. The first adverts to an attitude closely related to the philosophy of the Vienna circle. In terms of the developments of this paper, it is the failure to distinguish between the practice of correlations and that of symbolic explanation. Positivism is prone to look down upon the latter as meaningless and because of this never gets to the heart of any exact science. But Reichenbach obviously does not wish to go to the logical limit of denying the importance of theories. On the other hand, not venturing to concede to them the methodological distinction they deserve, he has to treat them by the method of correlation as though they were themselves observations, or a part of what we have called nature. Correlation, as we have seen, imparts diffuseness to the correlates, and hence there emerges the diffuseness, or probability, of theories.

The other aspect of our answer regards the scope of Reichenbach's system. It is clearly larger than that of all other probability theories, and it is made so by the universality implanted in the probability concept. This implantation, it is here maintained, is not in keeping with actual methods of physical procedure. But this universality lands its proponent finally in many-valued logic. The fact should be of interest to the philosopher that this destination is reached because of the retention in the system of elements, such as the probability of theories, which

have no scientific status, and that their removal would not lead to many-valued logics.

Lest the reader misconstrue our attitude as fanatical enmity against many-valued logics, we make these concluding remarks. New systems of logic are highly interesting and useful in their own right; their establishment need not be justified by the allegation that they are implied by scientific procedures at present. Their status is much the same as the work of Riemann, Bolyai and others on non-Euclidean geometry before concrete use was made of it in the theory of general relativity. Many-valued logics may someday revolutionize science. But their potential value would be impaired if confusion as to their bearing on contemporary methods were allowed to creep into their very making. For that reason the physicist's "optimum wager," to use Reichenbach's term, is to wait until the structure of many-valued logics is more fully developed, until their relation to two-valued logic is no longer in dispute, and until rules are given for applying that calculus to observable facts.

I record here my sincere appreciation of the benefits derived from discussions with Professor F. S. C. Northrop.

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Patterns of Culture in History

BY

SIDNEY RATNER

IF THE history of civilization is to be an instrument for the advancement of civilization, historians must break new paths toward a wider and deeper understanding of human affairs.[1] Despite the achievements of those who have explored the influence of technics, the power of nationalism, the rôle of mass-psychology, the economic factors in political and social life, our analyses of history are from a scientific standpoint still all too primitive. As John Dewey has said, "It is possible to study a multitude of histories, and yet permit history, the record of the transitions and transformations of human activities, to escape us. Taking history in separate doses of this country and that, we take it as a succession of isolated finalities, each one in due season giving way to another, as supernumeraries succeed one another in a march across the stage. We thus miss the fact of history and also its lesson; the diversity of institutional forms and customs which the same human nature may produce and employ."[2]

The prevalent confusion in popular thought between local custom and "Human Nature", based on a blindness to cultures other than our own, persists in our histories, and few historians

[1] Cf. Charles A. Beard, *A Charter for the Social Sciences* (New York, 1932); *Conclusions and Recommendations of the Commission on the Social Studies, American Historical Association* (New York, 1935).

[2] John Dewey, *Human Nature and Conduct* (Modern Library Edition, New York, 1930), 110.

on either side of the Atlantic show how many of the ways of behavior that we believe to be founded deep in human nature are actually expressions of our culture and subject to modification with our changing culture. A critical historian would show how each age and society has woven its social patterns, its institutions, out of the primary human drives or impulses under the most diverse conditions, with the firmest conviction that its ideals and standards were categorically determined by certain fundamental human qualities, fixed and unalterable.[3]

One central task of history, then, would be to reveal the plasticity of human nature, the way in which the basic human desires have found expression in different complex patterns of culture, the tremendous rôle that custom, institutions, and social norms have played in moulding man's "instincts". History would cease to be a study of the past as a dead object, an exotic or picturesque ruin. It would become a scientific inquiry into the ways human nature has been formed and transformed, an attempt to discover what insights we might gain from the experiments in living others have carried on before us.[4]

The first step towards that goal is an understanding of the pattern of culture within which we live and move and have our being. As Ruth Benedict has demonstrated, a culture, like an individual, is a more or less consistent pattern of thought and action. "Within each culture there come into being characteristic purposes not necessarily shared by other types of society. In obedience to these purposes, each people further and further consolidates its experience, and in proportion to the urgency of these drives the heterogeneous items of behavior take more and more congruous shape. Taken up by a well integrated culture, the most ill-assorted acts become characteristic of its peculiar goals, often by the most unlikely metamorphoses. The form

[3] Franz Boas, "The Aims of Anthropological Research", *Science*, (Dec. 30, 1932), 76: 605–13, and *Anthropology and Modern Life* (New York, 1928), 198–236; Muzafer Sherif, *The Psychology of Social Norms* (New York, 1936), 1–45.

[4] Cf. George Santayana, *The Life of Reason or the Phases of Human Progress* (2nd ed.; New York, 1927), 5 vols. No historian in Europe or America, to the best of my knowledge, has ever used this most brilliant study in the philosophy of civilization to illuminate any discussion of social values.

that these acts take we can understand only by understanding first the emotional and the intellectual mainsprings of that society."[5]

Cultures, like many other natural phenomena, are more than the arithmetic sum of their parts.[6] They spring from the union of diverse elements and have properties not possessed by any of their elements in isolation. That is why the synthetic method is needed to complement the analytic method: the analysis of complex systems, as Bertrand Russell once said, gives us the truth and nothing but the truth, yet it can never give us the whole truth.[7] Just as all the facts about water cannot be inferred from an exhaustive knowledge of hydrogen and oxygen, so a cultural configuration is not grasped until its many phases: art, science, religion, economics, social organization, politics—are seen forming a consistent pattern of integrated behavior.

Admirable as a principle may be in theory, its true worth can be seen only in the concrete cases exemplifying it. Professor Benedict has applied the principle of cultural patterns in a most illuminating study of three sharply contrasting cultures, each permeated by one dominant idea.[8] The Pueblo Indians of New Mexico exalt the calm, reasonable, co-operative life: theirs is the middle road between excess and asceticism. They hold power and violence in scorn. The individual sinks his activities

[5] Ruth Benedict, *Patterns of Culture* (Boston and New York, 1934), 46.

[6] "It is important to realize that a whole is a new single term, distinct from each of its parts and from all of them: it is one, not many, and is related to the parts, but has a being distinct from theirs." Bertrand Russell, *Principles of Mathematics* (Cambridge, 1903), 141. The classic definition of culture as "that complex whole which includes knowledge, belief, art, morals, law, custom, and any other capabilities and habits acquired by man as a member of society" was given by Edward B. Tylor, *Primitive Culture* (6th ed.; London, 1920) I, 1. Cf. Alexander Goldenweiser, *History, Psychology, and Culture* (New York, 1933) 121–66; B. Malinowski, "Culture, *Encyclopaedia of Social Sciences*, 4: 621–45; Bernhard J. Stern, "Concerning the Distinction between the Social and the Cultural," *Social Forces* (Dec. 1929), 8: 264–71.

[7] Bertrand Russell, *op. cit.*, 141. Cf. Morris R. Cohen, *Reason and Nature* (New York, 1931), 154, 249–82, 344–67; H. Levy, *Universe of Science* (New York, 1933) 31–81; Vilfredo Pareto, *Mind and Society* (New York, 1935), trans. A. Bongiorno and A. Livingston, I, 18–74; Charles Sanders Peirce, *Collected Papers* (Cambridge, 1931–1935), I, 27, II, 5–9.

[8] Ruth Benedict, *op. cit.*, 57–222.

in those of the group and contemns striving for authority. A sense of man's oneness with the universe dominates their world-outlook, completely foreign to the Western view of the universe as a struggle between the forces of good and evil.

In opposition to this Apollonian way of life, the Kwakiutl Indians of Vancouver Island led, not more than a generation ago, what Nietzsche aptly characterized as the Dionysian quest for intense experience, ecstasy culminating in the "annihilation of the ordinary bounds and limits of existence."[9] Their system of values centered on self-glorification. Conspicuous consumption and destruction of wealth was used as one means of shaming one's rivals. Their ideal was the man who succeeded in surpassing all others in bravado, arrogance, and ostentatious display and who would rather die by his own hand than suffer ridicule for failure.

A third cultural type is that of the Dobuans of Melanesia in the western Pacific. They consider ill-will and treachery virtues, regard jealousy and suspicion between man and wife as normal, and cultivate a feeling of fierce exclusiveness in all matters of ownership. Their scorn for laughter and other signs of happiness is equalled only by a dourness and prudery about sex that would do the Puritans credit. They believe man and nature to be hostile and malignant to them. All existence seems a life-and-death struggle against pitiless rivals for the goods of life.

These penetrating analyses of divergent cultures reveal the way in which the fundamental cultural attitudes mould individual and group behavior. Professor Benedict has shown how fruitful the study of folkways may be if approached from the viewpoint of their relation to the major goals and values of the culture. The implications of such an approach for the history of civilization are far-reaching, but not completely obvious.

Each civilization has its own cultural configuration, its dominant trends or emphases, its distinctive hierarchy of values. Empiric study is needed to determine the variety and complexity

[9] Friedrich Nietzsche, *The Birth of Tragedy* (New York, 1923), trans. W. A. Haussmann, 61. Cf. Walter Pater, *Greek Studies* (London, 1928), 1–41.

of possible cultural constellations.[10] Different cultures or civilizations vary, of course, in the degree of their integration and may contain conflicting drives. Some may fail to attain a consistent pattern; others may not be known in sufficient detail for us to understand the motives that actuate them. But most of the cultures encountered by historians seem to have achieved sufficient integration of thought and action for the application of this method. The problem is sometimes complicated by several complex sub-patterns appearing within the framework of a civilization; that is not, however, an insuperable obstacle to the approach suggested above.

In some cases, different elements from polar categories may be interfused. Santayana has stated that the Greeks (notably in Periclean Athens) achieved the glory and sweetness of being rational animals through a two-edged wisdom that made impulse the stuff of life and reason its criterion. "There is nothing exceptional in being alive and impulsive; any savage can run wild and be frenzied and enact histrionic passions: the virtue of the Greeks lay in the exquisite firmness with which they banked their fires without extinguishing them, so that their life remained human . . . and yet became beautiful: they were severe and fond of maxims, on a basis of universal tolerance; they governed themselves rationally, with a careful freedom, while well aware that nature and their own bosoms were full of gods, all of whom must be reverenced."[11] The whole marvel of Greek genius comes from this union of the Dionysian and Apollonian. Bacchic freedom and ardor were harmonized with a joy in order and loveliness, a sense for friendship, a reverence for sacred places and institutions.[12]

The range in form, quality, and value possessed by the great historic civilizations, past and present, cannot be easily grasped by one whose plasticity is limited by the very culture that has given him his vocabulary and his preconceptions about life and

[10] Arnold J. Toynbee, *A Study of History* (London, 1934), I, 133 presents a list of some 21 important historic civilizations.

[11] George Santayana, *Egotism in German Philosophy* (New York, 1916), 141–2.

[12] Santayana, *The Life of Reason* presents an elaborate study of this theme.

destiny. Fortunately, the anthropologists have been aided by a few pioneer historians who dared to blaze new trails that may yet become world highways. Among these pioneers was Wilhelm Dilthey, the noted German philosopher-historian. Aristotle had taught that everything ideal has a natural basis and everything natural an ideal development. Dilthey applied a similar insight to the history of thought from the 16th to the 19th centuries and showed how different philosophies of life and world-outlook were expressions of human genius, of varying emotion and impulse, poetic creations unprovable by pure logic, but powerful because they answered human needs and reflected the cultural *milieu* from which they sprang.[13]

In our own day the rôle that patterns of culture play in history has received its most dramatic and elaborate recognition so far in Oswald Spengler's noted prose-poem, *The Decline of the West.* His metaphysics is unsound, his historical parallels are often questionable, and his thesis that all cultures go through an inevitable life and death cycle is based on an untenable analogy between biologic organisms and cultural forms. Nevertheless, Spengler, as a disciple of Goethe and Nietzsche, has had some valuable and original insights. His principal contribution is that each culture has its own destiny-idea, a *Leben-und Weltanschauung* embodied in its specific configuration of ideas, emotions, and interests, an individual style differentiating its activities from those of other cultures. In his system classical civilization is characterized as Apollonian, and modern Western civilization as Faustian. The Egyptian view of life is presented as that of an inexorable prescribed journey in one unchanging direction, and the Arabic mind is envisaged "as strictly dualistic, centered

[13] Ruth Benedict, *op. cit.*, 52; Werner Brock, *An introduction to Contemporary German Philosophy* (Cambridge, 1935), 20–6, 123–4; and H. L. Friess: "Wilhelm Dilthey", *Journal of Philosophy*, (1929), XXVI: 5–25, furnish a good introduction to Dilthey's work. John Dewey, *Philosophy and Civilization* (New York, 1931), 3–12 presents a sociological interpretation of the history of philosophy that is comparable to Dilthey's. Morris R. Cohen, "Vision and Technique in Philosophy", *Philosophical Review* (March, 1930), XXXIX: 127–152, offers a valuable corrective to the abuse of the sociological method.

on the cosmic drama of creation and foundering, the struggle between Good and Evil, God and Devil."[14]

Spengler has undoubtedly oversimplified the cultural patterns he passes in review. Modern Western civilization, for example, cannot be adequately understood, on the psychological side, as the creation of the Faustian man, with his sense of life as a desperate conflict between his will and the world and with his intense longing for the infinite. The Faustian man is only one type among many in our cultural configuration. What of the Babbitts, the strong men of action, the extroverts "endlessly catching their trains"? The various sub-patterns that enrich and complicate our civilization have to be studied both in their geographical setting and their socio-economic stratification. Different social groups of the same locality and era differ radically in their way of life. The laboring masses and the plutocratic leisure classes have a common nationality and share certain folkways together, but their lives have different economic bases and are, within certain limits, oriented towards different cultural goals. Members of the same economic status may belong to different social and intellectual circles; the bourgeoisie, for example, includes the radical intellectual as well as the conservative Rotarian.[15] The worlds of Proust, of Henry James, and of Santayana's *The Last Puritan* revolve about the social élite, the cosmopolitan intelligentsia of Europe and America. Their protagonists are far removed from *la condition humaine*, as portrayed in Zola's *Germinal*, Hardy's *Jude the Obscure*, and Dreiser's *An American Tragedy*.

Critical historians will avoid the romantic and *a priori* methods that prevented Spengler's work from being more than a brilliant and magnificent *tour de force* and will test their insights by rigorous logical technique. They will strive to create valid syntheses of the diverse elements in each of the great civilizations into whose

[14] Professor Arnold J. Toynbee, *A Study of History*, I, 45; III, 378–90, agrees with Spengler that "societies, not states, are 'the social atoms' with which students have to deal" and that each civilization has its own penchant, bent, trend, or emphasis.

[15] Cf. Ruth Benedict, *op. cit.*, 54–5, 223–30; Thorstein Veblen, *Theory of the Leisure Class* (New York, 1899), passim.

rich and varied textures men have woven their lives throughout the course of recorded time. This task is most difficult. Most syntheses of the results of science have not been scientific, but dramatic, esthetic, *a priori* philosophical, or religious achievements.

The concept of patterns of culture offers, however, the basis for a scientific formulation, development, and application of the criteria of significance. It enables historians not only to integrate an entire civilization, but to attain the necessary psychological distance from the social norms that seem part of their very being, the perspective required for seeing the relativity of all cultural values and institutions, the possibility of other ways of life than those to which they are accustomed. Once they question their first principles and become truly universal and impartial in their thinking, their history gains in depth of insight and rises from the merely descriptive level to the analytical and reflective. Western folk ways and institutions: our competitive, acquisitive economy, reliance on war as an instrument of national policy, identification of sexual differences with supposedly innate temperamental differences, exaltation of romantic love, belief in a *Sturm und drang* period for all adolescents, stress on prestige, the will to power, the conspicuous consumption and destruction of wealth, for example, will be described in detail, but the other social patterns that have been or may be woven out of the primary human impulses will be indicated as a means of understanding and evaluating Western civilization.[16]

Not all historians, of course, have the genius that marks such works as Voltaire's *Siècle de Louis XIV*, Buckle's *History of Civilization in England*, Henry Adams' *Mont-Saint Michel and Chatres*, Elie Halévy's *Histoire du peuple anglais au XIX^e siècle*, Charles A. and Mary R. Beard's *Rise of American Civilization*, and Parrington's *Main Currents in American Thought*. But each historian, no matter how limited his field, can contribute much

[16] Ruth Benedict, *op. cit.*, 223ff; John Dewey, *op. cit.*, 106–171; Mark A. May and L. W. Doob, *Competition and Cooperation* (Social Science Research Council Bulletin No. 25, New York, 1937); Margaret Mead, *Sex and Temperament in Three Primitive Societies* (New York, 1935) *passim;* Muzafer Sherif, *op. cit.*, 113–204.

of value to the many-sided understanding of the civilization or civilizations he is most interested in by studying his subject in the light of its implications for the cultural pattern of which it is a part. Justice Holmes, for instance, in his classic volume on *The Common Law* analyzed the law as a great anthropological document embodying the story of a nation's development through many centuries and reflecting the dominant social trends and pressures of different ages.[17] In the realm of science, Erwin Schrödinger, the founder of Wave-Mechanics, has stressed the influence of the cultural milieu on the scientist's choice of problems and methods of research. The emphasis in the arts and crafts on simplicity and purposefulness, in philosophy and ethics on the relativity of standards, in social and industrial organizations on mass control, rationalization, and statistics is paralleled and reflected in contemporary scientific theory and investigation.[18] Scientists like other men are children, as well as moulders, of their age and are influenced by the general world outlook prevalent in their day.

The quest for the rational structure of history does not end with the recapture of the past in all its freshness and individuality, even with the discovery of the intellectual and emotional mainsprings of different societies. To understand completely the civilizations of the past and present we have to know not only what they are but how they came into being. Climate, race, individual genius, sex drives, the economic system, technology, art, science, and religion—all are elements in social change, and each has had its champions as the sole or primary cause of social institutions.

The dynamics of history: the study of the laws governing the emergence, flowering, decay and transformation of cultural pat-

[17] Justice Holmes also saw that "the law of fashion is a law of life. The crest of the wave of human interest is always moving, and it is enough to know that the depth was greatest in respect of a certain feature or style in literature or music or painting a hundred years ago to be sure at that point it no longer is so profound." *Collected Legal Papers.* (New York, 1921), 210.

[18] Erwin Schrödinger, *Science and the Human Temperament* (New York, 1935), 27–38, 81–132.

terns—still remains to be adequately explored, however.[19] One reason is that the revolutionary developments of modern physics have rendered most accounts of causality inadequate and outmoded. Another is that no one element in the human scene can be singled out as the only cause since all the factors present in any given situation are necessary and none in itself is sufficient. A third is that the relative importance of different social forces can be established only through careful analysis and measurement of their effect in different cultures. Here, as elsewhere, the concept of patterns of culture advances the scientific study of society. It offers a frame of reference for "brute facts", the seemingly blind, uncorrelated flux of events, and makes them subject to scientific comprehension.

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[19] Cf. Franz Boas, *Anthropology and Modern Life*, 131–63, 198–236, and "History and Science in Anthropology," *American Anthropologist* (Jan.–March, 1936), 38: 137–41; J. F. Brown, "Towards a Theory of Social Dynamics," *Journal of Social Psychology* (May, 1935), 6: 182–213; Morris R. Cohen, *Reason and Nature*, 333–68; John Dewey, *Logic* (New York, 1938), 442–512; H. Levy, *Science in an Irrational Society* (London, 1934), 34–80; V. Pareto, *Mind and Society*, III, 1406–32, IV, passim; A. J. Toynbee, *A Study of History*, II–V, IX–XI, (only the first 3 volumes of this 13 volume project have been published so far); and Thorstein Veblen, *The Place of Science in Modern Civilization* (New York, 1919), 56–81, 409–56.

DISCUSSION (CORRESPONDENCE)

In this section we shall publish brief communications commenting on published papers or bringing up special points of interest which can be discussed by correspondence.

THE MASS PRODUCTION OF RESEARCH

Dear Sir:

A very significant item appeared in the American weekly news-journal of scientists called *Science* for November 26, 1937. Perhaps "epoch-making" would not be too strong a word to describe it. For it is a brief article by a scientist in which the writer pleads not for increased scientific publication facilities, but for a wiser use of those now available. The writer, O. A. Stevens, of North Dakota Agricultural College, declared that delay in scientific publication was not always so serious as it seems and that the increasingly vast array of published material threatened to swamp research workers.

He condemned the multiplication of small journals of overlapping subject matter, which goes on constantly. He was outspoken in saying: "The chief restriction on the scientist's publication is the publisher's profit or the bank balance of the technical society." He declared that modern facilities for indexing and abstracting found it a hopeless task to cope with this mass of print and that quality rather than quantity should be stressed.

This amazing little article is almost unique as well. For, while there is endless discussion of securing increased appropriations for scientific publication and improved technical facilities to distribute socalled scientific knowledge, the problem of whether all this technical material is fit to print is usually altogether ignored. A convention has grown up to the effect that the results of all scientific research must be reported in technical papers. It has become customary to regard such papers, expressed as they are in language no mere laymen's intellect can penetrate, as very important and noble things.

The high quality of a technical paper is usually accepted without argument. Things are not so in general literature, where a very careful

selection of material is made. If you can believe the customary polite euphemy of the quality-magazine editor, your contribution is never returned for lack of merit but solely because he is so deluged with matter of such ineffable quality that he must discard your offering in tears.

So huge is this flood of scientific enlightenment that the situation appears to be creating alarm among conscientious research workers and professors who feel obligated to keep up with progress in science. Unfortunately the ruling hierarchy of science professes to be disturbed only because publication funds are limited and not at all because such a large percentage of trash appears in the decorous pages of impressive journals. It is no longer physically possible to combine thorough laboratory investigation with thorough *re*-search. The monograph is even rendered futile.

It is time that lay attention was called to this crisis. Laymen after all generally supply the money to finance research. Institution staff directors are appointed; they in turn appoint assistants and assign problems. The subscientists get to work in their laboratories. Ultimately the monetary contributors and the institutional directors want some tangible evidence of results. What better to impress them than a few pounds of incomprehensible printed matter?

Hence laboratory workers take time out every now and then to write papers in which they report their findings, decorated with graphs, curves, bars, tables, and pictures. Ultimately their paper, or something like it, is approved on up the line for publication, for too much disapproval would amount to an admission of palpable inefficiency from director down. The paper is then offered to some highly specialized journal with an arrestingly long name like a slow clap of thunder.

This journal has an unpaid editorial board, no member of which is perhaps in the narrow specialty in question, but all of whom hope in time also to publish their papers. For the urge to print is grounded in a natural desire to gain prestige, to assure priority, to ensure a living. The laboratory producing a full five pounds of printed matter annually can more reasonably expect continued financial support than one producing only four pounds and nine ounces. The status of individual workers tends to vary in direct ratio to their output in print-pounds.

The journal editorial board will usually have discretionary, not mandatory, powers. The journal will appeal to few readers, it will carry almost no advertising, and its subscription price will be very high. Its readers are customarily its contributors, hence also its subsidizers.

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It is solemnly true that scientific journals cannot today possibly print all the articles offered to them, in spite of the fact that research workers piously deplore the injustice of this,—and no wonder.

Yet any editor of scientific publications knows—and the present writer has been one of the breed for a decade—that all which appears expressed in esoteric and but faintly grammatical language is not contributory to science progress. A great deal of it is plain trash. But new journals are founded weekly. If the existing biological journals are pressed for space then special journals must be founded to cover nutrition, growth, food research, and so on—and they have been. One devoted solely to vitamins may appear now any day.

Ten or twelve years ago the Bishop of Ripon gained notoriety by asking that research take a ten-year holiday. Nor was he altogether misguided. For the late Sir Ernest Rutherford, when President of the British Royal Society, told an audience at the University of Bristol about that time that he was always hoping for a breathing space when, for a few years, no scientific advances of consequence would occur. He wanted to catch up with things but he added: "Alas, that breathing space has never come, and I am sure it never will come in my time."

Certainly it has not appeared yet. There is mass production of "research" today as of automobiles. Theses and hypotheses are intricate and complex. Factual data occur in abundance. The resources of mathematics are taxed. Yet quickly hashed-up papers that were rushed to print tend to undermine all. Papers accumulate in great snowballs immediately any basic observation is made by a gifted worker.

William Mansfield Clark of Johns Hopkins is such a worker. His researches in the remote field of hydrogen-ion concentration were outstanding. In 1910 only 50 papers were published on this subject. Then Clark got under way and the number of papers began to double about every 4 years. The total for 1929 was 1,500 and an annual production of 2,000 was imminent. Vitamins and ultraviolet rays brought in train whole libraries of trash papers also.

A distinguished American physician, Dr. Ernest E. Irons, speaking a while ago of the specialized medical technique of using foreign proteins in shock therapy, said excessive papers had been published. Many of these gave the impression that "if they had been laid away six months, they might not have been published, or the conclusions might have been modified." There were only 4 articles on the subject in 1916 but 63 appeared in 1922 when the subject was the height of fashion. Later

the subject lost its charms and the number of papers dropped to about 20 a year. The imitators were tired.

Seven years ago the great physiologist Krogh told the Thirteenth International Physiological Congress that, while German physiological abstract journals epitomized only 3,800 articles in 1901 the number was 18,000 in 1926. Only 100 or 2½ per cent of these papers came from Americans in the former year while 3,500 or 20 per cent came from them in 1926. In 1932 there were 200 journals devoting practically their entire space to clinical pathology alone.

In 1936 an enterprising scientific worker delineated in *Science* what he glibly affected to call the "progress of chemistry" by drawing graphs to plot the number of papers published in this field since 1907 when *Chemical Abstracts*, the epitomizing journal for chemists, was founded. That first year condensed summaries of 7,975 articles were printed in *Chemical Abstracts*. The second year the count mounted to 10,835 and in 1913 the figure was 19,025.

Then came the War. There was a recession. But progress got under way again in the form of the scientific urge to print. In 1923 the papers abstracted numbered 19,507. In 1935 the number was 42,468! If such "progress" could but continue the excited investigator now calculated that 135,000 articles in the field of chemistry alone would appear in 1985 and be republished in condensed form! But is that progress or disaster?

The mere task of sorting the trash from the few authentic contributions to scientific knowledge in such a mass of abstracts would deprive the conscientious scientist of all opportunity to do any laboratory work whatever. How could he tell which work was new and significant? How could he have any complete knowledge of what had already been published? How could he detect whether these so neatly reported experiments had been competently organized and performed?

In 1935 about 36,000 scientific journals were published throughout the world. These appeared in 18 languages though nearly half were in English. They were weeklies, monthlies, and quarterlies and vied with several thousand technical books and monographs which annually demand the attention of scientists. It takes about a thousand standard magazine issues daily to report what is ambitiously regarded as "science progress." If a worker confined himself to English alone he would have to read about 425 issues a day to keep up with "science."

This sort of thing has gone on to the extent that there is authoritative

complaint regarding technical journals founded purely for profit. Scientists have suggested the boycott of such journals. For knowing that individuals, institutions, and libraries always hesitate to "break a set" of a journal, the publishers boost prices to exorbitant figures and exploit scientific publication for profit.

Now since thousands of future scientific workers are going to spend thousands of hours wading through this mass of paper should not some present steps be taken to guarantee that it is other than a confusing cloud of symbols? No wonder one of the newer scientific journals, *Growth*, announced this somewhat curious editorial policy:

"Acceptable work will be published promptly with no restriction as to length. To accomplish this each paper must be accompanied by an order for 100 reprints at a cost of $3.75 per page of 500 words, additional reprints at small cost. Tables, illustrations, entail the usual extra charge."

The subscription rate for *Growth* was already $3.50 a volume of 400 pages. Its policy apparently means that if some long-winded scientific brother has three or four hundred dollars, or can induce the institution for which he works to expend that much, he may grow verbosely redundant to the extent of a hundred pages about his research exploits. Possibly this obligatory reprint-purchasing policy will cut him off short, but the contributions so far printed scarcely support this view.

A scientist without his reprints would be like a blacksmith without an anvil. For scientists are not content merely to have their papers published. They must also have fifty or a hundred reprints or separates of it with which to splash their colleagues. This is called the "reprint plague." These reprints the worker discharges at his fellows (or even at innocent strangers) either one at a time or, if he be prolific, in large bales, which he heaves rudely through the mails to explode bomblike on the desks of other research workers who did not ask for them—but who do the same thing themselves.

The English physicist, W. H. George, calls these fellows "paper merchants" or "paper machines" in *The Scientist in Action*. George also remarks that as soon as any general scientific principle is unearthed or some new constant is determined, a vast deal of "pump-handle," "safety-first," or "pot-boiling" research springs up among the lesser laboratory dignitaries. For:

"Given the requisite skill, the greater the industry in this kind of research, the more the established results (facts) just as under normal conditions, the water flows so long as the pump handle is worked."

Meanwhile our doctor-of-philosophy machines grind on relentlessly. High-power Ph.D. production continues regardless of demand. Now and then some astute and candid person, like Dr. J. McKeen Cattell, stops to decry this kind of mass production and to say that there is a far too rapid increase in the number of doctors, both on a numerical and a ratio-to-population basis. But nobody pays much attention to his incontrovertible statistics, or even to his assertion that the number of really distinguished scientists appears to bear an inverse proportion to the number of doctors of philosophy.

The degree no longer signifies that the recipient has enlarged or modified the field of human knowledge or presented some new and significant interpretation of facts. It evidences no particular mastery of a special field of study. It certifies little more than that the individual has done three more years university work after becoming a bachelor. But these doctors must be kept busy somehow, so it is natural that too many of them undertake research. Many of them work and publish trash continually on worthless or duplicating projects.

But enough destructive criticism. What is the remedy? The suggestion that papers be culled for quality is rarely if ever made. Instead the use of microfilms and other mechanisms is suggested more widely to disseminate what is chastely called "scientific knowledge." Now and then someone hops on scientists for their redundancy. It is said that they use too many words, graphs, tables, and illustrations, and that they indulge in circumlocutory English to such an extent that the average paper is fully one-fifth longer than it needs to be. Sometimes it is true that the scientist does give extensive, detailed treatment to his results simply because he had found it impossible to keep well-informed about progress in his specialty, and had a mistaken idea of their importance.

But these are, after all, trivial matters. The important thing is to shut off the stream of hurried, ill-prepared articles which often represent poorly planned or essentially worthless laboratory research work. Our present paramount need is some central agency, say a National Institute of Research, to act as a clearing house, and to render scientific investigation and publication both less costly and more efficient.

For that matter the government of each country, especially our own with its lavish research funds, should establish such a clearing house. It would be appropriately staffed with professionally qualified civil service employees who would do nothing but record and file, abstract and index, the books and journals published, and keep track of research

projects under way throughout the world. This labor would be carried on under the supervision of a scientific board representing every branch of science.

The board would not have dictatorial powers. Its functions would greatly resemble those of the Office of Experiment Stations of the United States Department of Agriculture, or of the various bureaus of agricultural science in Great Britain. It would induce scientists throughout the country to coöperate voluntarily in the formulation and application of its program. Such an institution would do great things to promote scientific progress, while at the same time greatly reducing the cost of research and scientific publications. The funds so saved could be put to good use.

Assume such an institution existed. A scientist thinks of undertaking a new research project. He communicates with the clearing house to orient himself. He is told the exact status of this problem the world over, how it is related to other problems, whether methods and equipment exist with which to attack it effectively, what other workers are engaged upon it, and what work they have done—published and unpublished. For all workers would have to keep the clearing house informed by means of frequent progress reports.

The inquiring scientist could also get from the institution a complete bibliography of the really significant publications to date with the chaff weeded out. He could be acquainted with material in unprinted notes. Thus duplication of effort would be prevented. Embarkation upon a hopeless problem would be forestalled until such time as new apparatus or techniques were available. The work of each scientist and each institution would be integrated with that of every other one.

If the project seemed important, means existed to attack it, and the particular individual and institution appeared capable of getting somewhere with it, a grant of funds and a license to attack the problem would follow. When the publication question ultimately arose a license to print would have first to be procured. Nothing could appear in the few basic journals of science, devoted exclusively to definitive expositions of new and fruitful scientific theories, unless this were fully warranted.

No paper could be printed merely because it reported doings in a laboratory in highly resistant technical language. The institution for which the scientist worked and the board of the clearing house would decide also whether the material rated a note of five hundred words, a short article, a definitive dissertation, or whether it should be ephem-

erally mimeographed, or merely filed in half dozen repositories in typewritten form.

Scientists rightly detest what they regard as regimentation, though they are prone to mistake mere rudimentary organization for something more sinister. They must consent to something along the line indicated if scientific progress is not to stall in a jam of mistreated wood pulp. The work of such a clearing house could be carried out without friction by making all American scientists copartners in its progress and policies. Coercion could be avoided, just as Dr. James T. Jardine, Chief of the Office of Experiment Stations has avoided it in securing the voluntary coöperation of all State agricultural experiment stations in his programs to prevent duplicating scientific projects being started in the field of agriculture.

The establishment of such a National Institute of Science would promote the voluntary coöperation of scientists for the greater good. It would effectually stimulate scientific progress. Above all it would rectify the current increasing evils of uncontrolled scientific publication.

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A REALISTIC CRITICISM OF A CONTEMPORARY PHILOSOPHY OF LOGIC

Dear Sir:

In his article on "Some Theses in the Philosophy of Logic," Dr. Nagel makes three points which together form the nucleus of his philosophy of logic: (1) discourse is a calculus and the subject-matter of formal logic is discourse so viewed. (2) The relation between instrument and subject-matter may be expressed by saying that the traits of the instrument indicate nothing about the traits of the subject-matter. In particular, the principles of logic are principles only of the instrument of discourse, and indicate nothing at all about the world in which the instrument functions. These principles of logic which Dr. Nagel identifies with the principles of inference are ways of effecting a transition from one piece of discourse to another. (3) Theories do not represent a hidden inaccessible reality; they formulate connections between the statements of everyday experience, the so-called singular statements. There is thus a sharp dichotomy between theoretical and ordinary statements.

Our criticism is confined to these three points which form Dr. Nagel's

philosophy of logic. In the first place, we believe that Dr. Nagel's treatment of discourse or language is totally inadequate for understanding how it functions in understanding natural relations, and that to merely say that the subject-matter of logic is discourse is incomplete for understanding how logic functions in the same process. The second problem which confronts us is to resolve the difficulty of the relation of language to its subject-matter, and in this way clarify Dr. Nagel's identification of the principles of formal logic with those of inference. In the third place, we do not agree with Dr. Nagel that there is as sharp a dichotomy between ordinary and theoretical statements as his view implies.

Before attacking the first problem—the discussion of the nature of discourse and the subject-matter of formal logic, we must distinguish clearly between discourse as a process or instrumental and discourse as a relatively stable instrument. On the one hand there is the general process of discourse, that linguistic activity which is universal to all men, because all men communicate, although the means by which it is carried on varies in different places and cultures. In this sense, language, or better languaging (since speaking and writing are activities) is a process or function analogous to the process of chiseling. This process is instrumental because it functions for two ends: one a metaphysical end, namely, a logical reorganization of the traits of the subject-matter of the discoursing; and secondly, a social end, communication.

By instrument we mean anything used *by* someone to *do* something *to* a subject-matter *for* some purpose. Although specific languages are not physical, they are, like chisels, instruments; because they are used upon and affect a subject-matter, namely, specific situations. Language as an instrument is an explicitly formulated or formulable system of symbols—what Dr. Nagel calls a calculus. To view language only as a calculus is clearly inadequate, because it does not take account of the process of languaging.

Language, like any other instrument, functions as a sign and thus possesses three general references which were partially indicated above by means of prepositions in our definition of an instrument. In the first place, it refers *to* some problem or purpose which directs its exercise and measures and interprets its effects. Secondly, it is an instrument *for* some aggregate of entities or domain of relations in which it is to be applied. Finally, language is an instrument in respect and because *of* its form, arrangement, or structure.

In concluding our discussion of the distinction between language as instrument and language as an instrumental process, we can state that languaging is the using of language as an instrument. The system of symbols is used for communication and logical reorganization just as the chisel is used in chiseling for dividing wood.

It seems to us that the distinctions between grammar, and formal and functional logic, or in Dr. Nagel's terms, pure and applied logic, should be drawn in terms of the above distinctions. The principles of functional logic are characters of all natural existence, because we all language in the same world. Just as the principles of chiseling are the principles of the subject-matter of chiseling, so also the principles of logic, that is, of languaging (whose subject-matter is all processes) are the principles of all natural existence. The relationship implied here between instrument and subject-matter will be discussed in the second section of our paper.

By saying that the principles of functional logic are characters of all natural existence, we do not mean to revert to the position which holds that logical principles are intuited a priori as indubitable and necessary. We believe that logical principles are discovered by an examination of languaging in the knowing process and therefore since knowing is a natural process the principles of logic are principles of a natural process. Even as the principles in other sciences which seek to make nature understood are empirically derived and are hypotheses, so the laws of logic are of this type—empirically derived. Such principles are illustrated in: (1) an object cannot both be and not be some specific thing, such as a table, (2) a table is a table, and (3) the object is either a table or not. Another such principle is illustrated as follows: if certain kinds of clouds and rain are in fact connected in such a way that clouds do bring forth rain, the appearance of these clouds makes further reference to their action unnecessary. Preparations for rain can be made without further attention to the clouds, because rain is functionally substitutable for clouds.

Since the language process is another natural process, these principles of natural existence can be found in it also. These principles are *formulated* in *formal* logic in terms of transforming one sentence into another. This transforming of sentences is a trait of natural existence in the sense that we are transforming one natural activity into another. When we say "clouds imply rain" we are obviously talking not only of word-clouds or word-rain, but of actual clouds and actual rain. Thus formal logic indirectly formulates rules of natural existence.

In formal logic the principles of functional logic become such transformation rules for sentences as "Not both p and not-p," "p is identical with p," "either p or not-p," and "p and p implies q together imply q."

The functional logician shows how these general principles as formulated in formal logic function in the instrumental process. He studies how the universal rules govern the usages of symbols in inference which is based on natural relations. He shows what kinds of *real* implication there are. The formal logician merely formalizes *all kinds* of implication as implication, and does not differentiate between types of implication. The functional logician indicates the different kinds of implications between things according to the kind of things related. To a formal logician $(a \cdot a \supset b) \supset b$ is valid irrespective of the particular nature of a and b; but the functional logician would distinguish at least four different types of implication: (1) that in which an object implies a function, (2) a function implies an object, (3) a function implies another function, and (4) an object implies another object. In fact, the functional logician must push this further and discover the categories into which a and b can fall in order that implication may be *actually* valid.

For example, there is a natural relation between a certain kind of cloud and a shower of rain. This falls into the first type of implication noted above. Physically, this is a causal relation; logically, it is two things: for formal logic it is ponendo ponens, and for functional logic it is a certain type of implying, meaning, or suggesting. When that type of cloud appears in the sky, rain is at hand, unless, by chance, the cloud is veered away by a wind. We infer from that type of cloud the natural phenomenon rain. The functional logician indicates how to put this inference, this suggestion or this meaning into symbols, basing it on the natural relation between cloud and rain. He says, in other words: "A certain kind of cloud always brings rain," "That kind of cloud is now in the sky," "Therefore, it is going to rain." The first premise is based on past experience, the second on observation, and the conclusion is merely a suggestion of what might happen.

However, formal logic and grammar are not identical, as Dr. Nagel implicitly holds. Because formal logic contains universal principles, it is not conventional; it differs from grammar which is conventional, for the rules of formation differ in different symbolical systems. By convention we mean a tacit or explicit agreement of people in a certain society to use a certain system of symbols according to certain rules. Thus the subject-matter of grammar is the instrument of language.

We are beginning to see more clearly than ever that it *is* necessary to

go behind discourse in order to clarify and justify the operation of the principles formulated in formal logic and operating in functional logic. The reason for this is that we cannot study any instrument *as* that instrument independently of its functioning. When we study a chisel independently of chiseling we do not study a *chisel* at all, but merely a piece of iron with a wooden handle. Contrary to what Dr. Nagel says, the formal logician does not study *discourse* as an instrument, but the *instrument* of discourse in isolation of the instrument's functioning. If discourse as an instrument is studied divorced from its functioning, no knowledge can be derived therefrom. Even as the study of money separated from its functioning as a medium of exchange is not knowledge of money as an instrument, but simply gives us knowledge of the material out of which money is made. The formal logic of the last 15 years tells us as little about discourse as a functioning instrument as nuismatics does about monetary systems. Formal logic has been a mathematical toying with the instrument. Like the Lilliputians blankly staring at the contents of Gulliver's pockets, but not understanding anything about the instruments, the formal logician has merely stared at the symbols of the instrument of discourse and rearranged them in his museum case. One recent museum acquisition has been the many-valued logics which were developed when mathematically-minded logicians, discontented with a mere two-valued logic, introduced n truth-values. The best that they can say to justify this activity is to say that these logics *might* have functioned in ordinary languages and that it is mere historical accident that a two-valued logic is in fact used. We must, however, point out that this two-valued logic is the only one which has been used by all men because the world is the kind of world which forces us to use a two-valued logic in understanding natural relations. As we have seen, the principles of formal logic have no meaning unless they are studied as they function in inferring which is a process dependent upon natural relations.

Now that we have discussed the instrument itself and the functioning of the instrument, we must turn to the relation of the instrument to its subject-matter; in other terms, to the relation of language to natural relations. We do not believe, as Dr. Nagel does, that there is no relation between the traits of the instrument and the traits of the material upon which it works. He would, for example, like to say that the principles of chiseling tell us nothing about the wood upon which the chisel operates.[1]

[1] This point was brought out by discussion with Dr. Nagel.

But it seems to us that the traits of the instrument are correlative with some but not all of the traits of its subject-matter; the traits of the instrument correlative with the traits of the subject-matter are those which it affects. The trait of one body is correlative with the trait of another body if both must be present in order for a certain end to be achieved by the interaction of the bodies. If we use a chisel upon a piece of wood, the sharp edge of the chisel is correlative with the traits of the wood which permit the wood to chip off. If we chiseled a cloud we would not expect to find any trait of the cloud correlative with the edge of the chisel, at least with respect to the end of chipping off; consequently the cloud can not be chiseled. The relation of the traits of the instrument to those of the subject-matter is that of actualizing the potential. The essential trait of a chisel is its sharp edge by means of which it can divide or split wood. The correlative trait of the wood is its ability to be divided or split. The trait of each is actualized only when the wood is split by the chisel.

To push this potentiality-actuality relation further, we note that the potentiality of the subject-matter depends upon certain traits of the subject-matter. For example, in order to be divisible or splitable, wood must be a continuous unshatterable solid with a surface softer than the chisel and not too small in dimensions. The correlative trait divisibility denotes a *function* of the subject-matter; the traits in terms of which it is explained are *structural* features of the subject-matter. In the process of actualization the instrument performs the function, utilizing the structural features of the subject-matter.

We find that the relation between instrument and subject-matter is that of alteration of each other by each other. Even as the chisel cuts the wood, the wood dulls the chisel. This interaction or functioning is what is responsible for the change in both instrument and the material upon which it works. It is this interaction which we have previously called actualization.

Dr. Nagel suggests that correspondence theories of truth are naïve, but he does not develop the relation between the instrument (language) and its subject-matter (situations). In the case of the language instrument and natural relations this interaction consists of two processes, inferring and languaging. The end of thinking is to select natural relations and infer on the basis of them. The end of languaging is a logical reorganization of the natural relations in symbolic form. The principles of inference are the same as those of languaging, namely the rules of functional or formal logic. Inference and languaging are not the same

process, but have the same rules. The processes differ in that languaging is external discourse whereas thinking is internal discourse. As we have seen, the rules of functional logic are extracted from the process of languaging and formulated in the instrument by the formal logician. It is only in this sense that it can be said that the principles of inference are identical with the principles of formal logic.

In the case of interaction between language and natural relations the effects of the interaction are clear. As the subject-matter is probed more deeply by the scientist, the theoretician makes clearer distinctions and new rules for formation in language. As the instrument is being altered by the subject-matter, the instrument is indirectly affecting the subject-matter, exploring it and elucidating its traits. The manner in which the instrument of language affects subject-matter is always through the medium of action. This action would not have taken place, or at least its significance would not be understood, if it had inadvertently taken place without the prior use of language. Examples from science in which such a phenomenon has occurred, can be multiplied. Suppose several geologists discuss the possibility of finding metallic ore in a certain region. Their talk leads them to the use of various "divining" devices and the ore is discovered, mined, refined, and utilized in industry. Surely language has altered the ore in that mine, although indirectly.

Dr. Nagel's position on our third problem of the relation between theoretical and ordinary statements is that the end of the former is adequacy, and their function connectiveness; while the end of the latter is truth and their function representativeness.

The first point that we wish to make is that neither ordinary nor theoretical statements have a representative function. We do not believe ordinary statements represent anything. Rather they are expressions of what is selected out of a subject-matter. For example, in the ordinary statement "the house is red" we are not representing the color of the house in any fashion whatever, but have selected color out of all the characteristics of the house and expressed it.

If Dr. Nagel uses the operational theory of meaning consistently he would not be able to call ordinary statements representative and really mean it. For in terms of this theory, ordinary statements as well as theoretical ones express the potentiality of things in that we can always find methods to verify them. To say that the house is red is to say that by approved scientific methods it can be discovered what the color of the house is.

On the other hand, consider a theoretical statement like the law of gravitation: "Bodies are attracted toward each other with a force inversely proportional to the square of the distance between the centers of the bodies." This statement does not represent anything, but expresses a certain trait that pairs of bodies taken in isolation exhibit. Thus neither kind of statement is representative.

However, as Dr. Nagel says, theoretical statements connect singular statements. But to say this is to view the problem only from the point of view of formal logic, that is, it merely states that one can deduce by means of theoretical statements certain singular statements. From the viewpoint of functional logic it appears that one would have to be able, not only to deduce singular statements, but also to infer certain *actions* which would confirm or disconfirm the theory. In other words, theoretical statements do not merely connect singular statements; they formulate connections discovered between actions. In more general terms, theories are instruments for understanding and understanding always leads to action; therefore there must be a direct relation between theory and action; one must be able to pass from inference to action just as Nilson passed to the discovery of scandium on the basis of Mendeléeff's inferences. Mendeléeff had noted a gap in the periodic system of elements between calcium and titanium. From the nature of the surrounding elements he deduced the atomic weight as well as many physical and chemical properties of the yet unfound element. Shortly after, Nilson isolated an element whose behavior was that of the predicted element.

Theoretical statements have no monopoly upon the trait of connectiveness. Ordinary statements may also formulate connections between activities or between things. For example, the statement that "if the sidewalks are wet, it must have rained," formulates the connection between the wetness of the sidewalks and a past downpour. Not all ordinary statements are connective, however. Such a statement as "the house is red" is not connective, because the "is" does not formulate the connection of the house to the red paint.

There is no sharp dichotomy between theoretical and ordinary statements as Dr. Nagel suggests, for all statements, theoretical and ordinary, are logical reorganizations of the traits selected from the subject-matter. This logical reorganization is expression. In logical reorganization there may be either temporal or spatial inversion of the selected traits. Even the simplest form of logical reorganization, the "if . . . . then" relation may be a temporal inversion. When we see

that the sidewalks are wet, we infer that it must have rained. We have reorganized the situation logically, passing from the temporal to the logical. In the converse case we would have "since it is raining, the sidewalks will be (are) wet." In this example the temporal order is not reversed but is maintained in the logical reorganization.

The difference between statements described by Dr. Nagel as connective and singular is thus recognized to be one of degree, not of kind. Connective statements are more complex reorganizations of natural relations than are the so-called singular statements.

As a consequence of this recognition of all statements as logical reorganizations of and correlative with the traits of their subject-matter, we can see that the only aspect of statements that is relevant to describing the relation between them and their subject-matter is the degree of reorganization which they effect, or the degree to which they are correlative with the traits of their subject-matter. Just as we speak of the adequacy of any tool, for example, a chisel, meaning thereby the degree to which it is a reorganization of its subject-matter, so we speak of the adequacy of statements. Just as we judge the adequacy of a chisel by how effectively it can be used for splitting wood, so we speak of the adequacy of statements, meaning thereby adequacy *for* use in some context of action. We thus go beyond Dr. Nagel who holds that it is more appropriate to speak of the adequacy of theoretical statements rather than of their truth or falsity, in extending this view to all statements. Looking at adequacy from another point of view we see that true statements are those which are adequate and false those inadequate, so that truth and adequacy become the same.

It seems to us that the self-corrective method of science makes its statements more and more adequate expressions of natural relations. Although we agree that there is no convergence to a fixed structure of things in a mathematical sense, we do believe that the degree to which statements are logical reorganizations of or correlative with natural relations has been increased in the course of the history of science. And if empirical laws and such ordinary statements as "water quenches fire" are practically invariant over centuries of research, it is because the degree of logical reorganization which has been attained in them is sufficiently high for use in the contexts in which they are formulated. It is not surprising, on the other hand, that theoretical statements should change so rapidly and seemingly whimsically. The context of theoretical statements shifts so much more frequently and also so much

more must be taken into account than in ordinary statements, even connective ordinary ones, that any reorganization of such an increasing multiplicity of the traits of the subject-matter is bound to be partial, inadequate, and easily subject to alteration.

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REVIEWS AND NOTES

DEWEY'S VIVISECTION OF THE LOGICAL PROCESS

Logic: The Theory of Inquiry. New York: Henry Holt and Company. v, 546 pp. $4.00.

That all normal human beings operate on the basis of some logic (however faulty it may be or fumbling their operations) is evident in their ability to trace and use connections between means and ends. Were they not able to do so, inference would not be among the actualities of human existence, and the conditions of valid inference would accordingly not concern them. This ability it is, however, which in lieu of suitable instinctive equipment enables them to *make* a living in a world which offers them only possibilities to be realized. This ability it is, too, which enables them to see problems where there might otherwise be only confusion, and in the process of seeing them to formulate ways of dealing with them satisfactorily. If the science of logic, then, is determination of the conditions of valid *inference* (which is only implication come to life), then one of its possibilities, at any given time, is to provide the most effective technic available for the solution of problems.

Believing that in this case the possible has become the actual to some extent, and must become increasingly so, John Dewey has performed a classic vivisection of the logical process. On the body of the logical tradition, modern logicians have shortened a leg here, grafted on an appendage there, and tried, without entire success, to do a job of face-lifting. And Dewey has taken this Caliban and shown that, despite his exterior, his organs are one with a world of continuous experience.

It is well to observe at the outset that this is a great book, great in the manner in which it brings together certain main currents from the past, great in its implications for further development and exploration. It is to our time what William James's *Principles of Psychology* was to his: so comprehensive in scope as to be sure of manhandling of details by specialists working in one branch or another of the science; and yet so in tune with a moving world of construction and reconstruction as to open up a new world of possibilities. From the viewpoint of a dynamic

philosophy, it is the most important contribution to American intellectual history (excepting, of course, the special case of Peirce's *Collected Papers*) since the publication of James's *Psychology*.

The reference to continuous experience indicates that much of Dewey's previous work is assumed in this one, since his *Experience and Nature*, for example, was primarily an examination of that continuity. But the manner in which it is assumed is at the same time an illustration of the conjugate relationship which he stresses throughout this work: for in the course of being assumed it is in a sense outmoded; it has provided the instruments with which this work is fashioned, and in the process they are re-fashioned. Perhaps a trite figure can be forgiven because of its patness: for fifty years he has been struggling with a big freight toward the light—now he has seen it. And that, somehow, makes all the difference.

Logic, as he has himself indicated elsewhere, was Dewey's first and deepest love. But two influences have been prime conditioning factors in his relationship to it. The first was the democratic vision, developed and expressed by Emerson, as it affected a Vermont Yankee. The second was the psychology of habit as worked out by James. From the first has stemmed his preoccupation with the common man and common sense. From the second has come his emphasis on the biological bases of human behavior. The consequence for logic has been that he has always seen it as having somehow to do with the activities of men in a world of problems.

The impact of James's *Psychology* led to the Darwinian emphasis in Dewey's thinking which helped to shape the early papers in *Studies in Logical Theory*. This was the formal presentation of the instrumentalist theory, that knowledge does not characterize all experience, but is instrumental in carrying the human organism from a problematic situation into an experience of having, enjoying. The matter was not entirely clear: certainly not to others, and evidently not to himself. He tried in two different ways, therefore, to make it clearer: first from the more psychological side in *How We Think* (which, from the viewpoint of a theory of knowledge, was as much an inquiry into *why* we think), and second from the logical side again, putting the original papers into the *Essays in Experimental Logic* with a long introduction and with a canvassing of some of their implications in connection with experiment, or scientific method. Still it was not clear. Evidently the matter went deeper than he had at first supposed, and the difficulty was at least twofold: the way in which knowledge was regarded in relation to experience,

and the language available in talking about experience. The way in which knowledge was to be regarded, he thought, was the way in which scientists regard it in their practice, as a reconstruction of the world in the process of knowing it: hence *Reconstruction in Philosophy*. The language difficulty seemed to offer a more complex problem, for it involved the question of making a detailed examination of the "reconstructed" position while using words with different denotations and connotations in "unreconstructed" philosophies: in large measure, this accounts for the difficulty of *Experience and Nature*. Insofar as this book made clear how knowledge-experience operates in relation to having-experience, however, the way was open for examination of the latter, a function performed by *Art as Experience*. And this last, in turn, concerned as it was in part with the relation of form to subject-matter, placed one again in the midst of a crucial logical problem.

How all these considerations work into the present *Logic* may be seen from a brief outline of its contents. Logic, Dewey contends, is not an affair of any fixed first principles, of some "pure Reason," lying outside the realm of experience. It has to do, rather, with the inquiries which determine the relation of means and consequences in experience. Logic is inquiry into inquiry. As such, it is an activity of living organisms, whose habit of inference is biologically grounded, and who only at a relatively late period become aware of this habit as a method of dealing with events. Now a habit is a way of acting, and if it is a way of acting for people generally, it is formulated into a principle of action. "If there are such habits as are necessary to conduct every successful inferential inquiry," accordingly, "then the formulations that express them will be logical principles of all inquiries." In addition to this biological basis of inquiry, there is the social milieu in which it is undertaken. It is this social factor which makes a reform of logical theory imperative. For the scientific revolution has made the contours of our society totally different from those of ancient Greek society: and just to the degree that the logic of Aristotle was adapted to that society, by so much is it unfitted for ours.

The effort to maintain the forms of Aristotle's logic, in the face of such intellectual change as the denial of eternal species, for example, has been responsible for treating logical forms as merely formal: with the result that false disjunctions are created between common sense and scientific method, between scientific method and logic. Logical forms are not an independent kind of being into which existence is to be fitted if possible;

they are forms *of* subject-matter in existence "when the latter is subjected to controlled inquiry." It is a mistake to believe that inquiry is a subjective process. It is an operation, or series of operations, with subject-matter that is objectively observable, and the process itself, therefore, can be objectively evaluated. This is evident if one keeps in mind that inquiry is occasioned by an indeterminate situation—again, indeterminate objectively. When inquiry is begun, the indeterminate situation is shaped into a problem. But it could not even be shaped into a problem if the original situation did not have some definite constituents. These constituents, determined by observation, suggest a solution: but it *is* a solution only if it functions as one, and the test is whether or not it transforms the indeterminate into a determinate situation.

This settled outcome of inquiry, this resolution of an indeterminate into a determinate situation, is judgment. Judgment, however, is intermediate as well as final in the process of inquiry, for it is the evaluation of propositions that suggest themselves as possible solutions —in this sense of evaluation, value judgments enter into the formation of all final judgments. If it is asked whether questions are propositions, the answer is that all propositions "have an interrogative aspect. Since they are provisional, they are not only subject to being questioned but they themselves *raise* questions of pertinency, weight and applicability."

It will be seen from this that propositions are regarded as strictly functional in the logical process. And in their functional rôle, they operate conjugately. In relation to propositions, judgment itself functions conjugately, both distinguishing and relating subject and predicate. The conjugate relation of contrary propositions, for example, is evident if they are viewed functionally: if the "vicious intellectualism" of rigid either-or alternatives which misapprehend a moving situation is to be avoided, contraries must be recognized as operating jointly to permit more discriminatory determinations of a situation. So all propositions involving judgment are both narrative and descriptive: narrative in that they concern a temporal course of events, descriptive in that events occur in space as well as time.

The functional nature of propositions has particular relevance to the consideration of generals. The hypothetical nature of universals is commonly recognized. But there are two kinds of generals, only one of which is properly universal. This is that kind which formulates a way of acting or operating. The other type of general proposition, having to do with kinds, classes, or genera, is generic. This, too, is functional,

since "the traits which descriptively determine kinds are selected and ordered with reference to their *function* in promoting and controlling extensive inference." But the attempt to reduce generic propositions to universals is fallacious, and responsible for much confusion in logical theory. They bear a conjugate relation to each other, but are not at all the same type of proposition. A universal is such a formulation of a mode of action "as serves to direct the operations by means of which existential material is selectively discriminated and related (ordered) so that it functions as the ground for warranted inferential conclusions." A generic proposition is the ordering of existential materials. A universal is the means of *grounding* any proposition logically.

Furthermore, the functional approach implies the throwing overboard of the traditional definition of a proposition as that which is true or false. A proposition is a means of dealing with a situation. But means are never true or false. "Means are either effective or ineffective; pertinent or irrelevant; wasteful or economical, the criterion for the difference being found in the consequences with which they are connected as means."

This realization of the functional nature of propositions shows that logic and scientific method are all of a piece. It is not true, as is sometimes said, that logical principles which have been developed apart from experience are then "applied" in scientific method. On the contrary, insofar as scientific method is effective inquiry into existence, it *is* logic. There is, accordingly, no difficulty in accounting for mathematics on functional grounds. Mathematics is simply inquiry carried one step further: "the function of abstractior which is involved in all existential inquiry being itself abstracted and universalized." Isomorphism is the key to understanding how this abstraction of abstraction can prove useful in experience.

The need for reform in logical theory and for the conjugate use of contraries has already been stressed. In no respect are they more necessary than in the case of induction and deduction. Separation of logicians into inductive and deductive camps has resulted in the impoverishment of both. If the operational view of inquiry is adopted, however, it is seen that it is not only desirable to use them conjugately, but that it is impossible to draw a sharp line of division between them. The inductive process is nothing other than the operations by which an indeterminate situation is transformed into a problematic one, with the solution suggested for its further determination. The deductive process is simply the use of universal propositions for grounding a solution logi-

cally in order to employ it for determining the situation. Conjugately, again, inclusion and exclusion, affirmation and negation, play indispensable rôles in the deductive process. And the entire hypothetical-deductive stage occupies an intermediate position in inquiry.

The confusion which has characterized this and other problems has been particularly active in bedevilling the problem of causation. If causation be defined as a necessary sequence of events, it involves an insurmountable difficulty: only universal, non-existential propositions are necessary; the existential falls within the category of probability. If the matter is looked at functionally, certain scientific "laws" are seen to be universals, from which no existential proposition can ever be derived. Necessity holds with respect to these, but on the other hand sequence does not, since being non-existential they are also non-temporal. On the side of events, sequence holds where necessity does not. It is evident, therefore, that causation as necessary sequence has no grounding in existence apart from inquiry. It has relevance only to particular inquiries, and as such is a logical category. "An event has to be deliberately *taken* to be cause or effect. . . .Given the problem of resolving a gross and indeterminate succession of observed qualitative events into a single continuous history, there is sufficient and necessary ground for taking one event as 'effect' or consequent, and some other as antecedent or 'cause'."

This clarification of what is meant by causation is essential if social inquiry is to proceed more successfully than it has up to the present time. Inquiry in the social field has not achieved the high degree of probability in its solutions that the physical sciences have, partly because it has mistaken some of the tools of inquiry in these sciences for a method of inquiry. Once it works with the proper method, it will fashion better tools for dealing with its own problems. For scientific method springs from the same roots as common sense, following the same general pattern of inquiry, but with secondary differences in technic because it has set itself different problems. Development of the social sciences, too, is ahead as it works out its specific secondary technics, but in accordance with the "canons" already discussed, it must first become aware of what the method really involves.

Thus in this last, as in so many of his earlier works, Dewey appeals again for the use of scientific method in solving social problems. From the individualism which characterized the democratic vision in Emerson and James, there has run the steady current in his work of endeavoring

to make that vision social. He has attempted to do so educationally as he has psychologically and politically. And there has been a certain grim determination (both *of* the problem and *in* his attitude) in the manner in which he himself has conjugately used contraries, between the institutions which will not become social unless men make them so, and the men who are hindered from becoming social by the nature of their institutions. The hope remains for him what it has been: scientific method.

There is, however, an indeterminate situation even here which requires problematic formulation before it can find a solution. For Dewey has observed that common sense and scientific method spring from the common matrix of a problematic world, and have the same general pattern of inquiry. Though they have different secondary technics, contingent upon their different aims—common sense aiming at use-enjoyment, science at existential relations—it would seem that this is not sufficient to account for the wide chasm that has come between them.

From one point of view, the individual is social to the degree that he can genuinely communicate with others of his kind. Now communication is an affair of having experiences together: primarily, though not exclusively, abetted by language. It is also an affair of having things together. The products of scientific method have been new materials for use-enjoyment: how is it that common sense has not been able to promote their use in common? How is it that things which could promote communication among people (but up to the present have done so only superficially) have to a degree hindered communication? Part of the answer might lie in the fact that common sense is an ambivalent term: on the one hand, it stands for a "practical," or instrumental facing of problems, and in this sense its pattern of inquiry is that of scientific method; on the other hand, it stands for the way in which people commonly see things, and in this sense it is separated from the dynamics of scientific method. It might be submitted, therefore, that whatever relative evaluations are placed on the forces at work in society, the mechanisms which science has provided will never be controlled until people really see things differently. Looked at in this way, therefore, it would appear that an important factor in making the socially indeterminate situation a solvable one is not so much scientific method as the development of a new common sense. Not least among the virtues of this *Logic* is that it enables people, therefore, to bite more effectively into the social situation.

The continuum of experience to which Dewey was working in *Experience and Nature* has found its culmination in this examination into the continuum of inquiry. The influence of Peirce in this and other respects is evident and acknowledged, and it is pleasant to find this most seminal of American minds helping to make clear the ideas which have fashioned a great book. For its appeal of the moment may be limited to those who have a technical interest in logic; but its greatness will be realized when the instruments it provides have become the common property of common men.

JEROME NATHANSON.

BOOKS RECEIVED

Fearfully and Wonderfully Made. The Human Organism in the Light of Modern Science. By Renée von Eulenburg-Wiener. New York, The Macmillan Co. 472 pages, $3.50.

A competent account of the functions of the human organism, its physiology, anatomy and behavior as a whole. The introduction is in praise of the use of speculative philosophy by scientists. The author confuses materialism with mechanism, and does not realize that for about 100 years Hegel has been completely stood on his head. Nor has physics eliminated "matter" (for which see Einstein's and Infeld's "The Evolution of Physics") nor has it thrown the balance in favor of "mind", "free will" or similar idealistic "foundations". Otherwise the book is an excellent popularization of biochemistry, physiology and medicine.

La Psychologie Expérimentale en Italie. Ecole de Milan. Par A. Manoil. Librairie Félix Alcan, 489 pages, 80 francs.

The University of Missouri Studies. Volume XIII Number 2 April 1, 1938. University of Missouri, Columbia, Mo. Contents: Distance Geometries, A study of the development of abstract metrics, by L. M. Blumenthal.

Culture Conflict and Crime. By Thorsten Sellin. New York, Social Science Research Council. 116 pages. $1.00.

A report of the subcommittee on delinquency of the committee on personality and culture. The author discusses the tentativeness of scientific conclusions, and is, therefore, quite cautious about techniques, facts and interpretations. Much is outlined for future research.

L'Ideologia Rosminiana Nei Rapporti Con La Gnoseologia Agostiniano-Tomistica. By Grazioso Ceriani. Universita Cattolica Del Sacro Cuore. Milano. 410 pages. 25 lire.

Vision and Organization, Periodicity in Social Structures. By Francis J. Mott. Mass., A. A. Beauchamp. 242 pages. $2.50.

A very sincere effort to build up a philosophy on the clue furnished by chemistry, as exemplified in Mendeleyeff's periodic table of the elements. In trying to transcend both materialism and idealism the author arrives at a clear separation of mechanism from materialism, as is illustrated by Marxism. For class warfare this author substitutes a kind of social experimentation, which, to this reader, seems to be an elaborate evasion of the necessity of revolution.

Are We Movie-Made? By Raymond Moley. New York, Macy-Masius. 64 pages. $1.00.

This book is mainly devoted to a demonstration that a set of researches, supported by The Motion Picture Research Council, purporting to prove that the movies caused crime and delinquency, were badly biased and unscientific. The shoddiness of such researches has been apparent almost at a glance to those trained in the natural sciences. Mr. Moley claims that he needed the inspiration of a study by Mortimer Adler to wake up to the danger of pseudo-science.

Encyclopedia and Unified Science. By Otto Neurath, Niels Bohr, John Dewey, Bertrand Russell, Rudolf Carnap, Charles W. Morris. International Encyclopedia of Unified Science, Volume 1, No. 1. Chicago, The University of Chicago Press. 75 pages. $1.00.

This will receive special review.

Foundations of the Theory of Signs. By Charles W. Morris. International Encyclopedia of Unified Science, Volume 1. No. 2. 59 pages. $1.00.

This will receive special review.

Procedures of Empirical Science. By Victor F. Lenzen. International Encyclopedia of Unified Science, Volume 1. No. 5. Chicago, The University of Chicago Press. 59 pages. $1.00.

This will receive special review.

Scientists Are Human. By David Lindsay Watson. England, C. A. Watts & Co. Ltd. 249 pages. 7s.6d.

While pointing out that scientists are affected by their culture, this author slips in many tributes to vagueness, poetry, the unique, and to mystical roads to knowledge. There is an avalanche of exaggerations and invective, portraying scientists mostly as the naivest kind of fools who do not "understand" that they have not explained everything.

Unity of Science Forum. By The International Institute for the Unity of Science. Holland. 8 pages. 2½ Dutch Guilders for twelve issues. August, 1938.

This is a section of the Dutch periodical "Synthese", 5 Raboes, Laren-Gooi, Holland. Contains a report of the fourth international congress for the unity of science, devoted to the discussion of scientific language.

Bayle's Relations with England and the English. By Léo Pierre Courtines. New York, Columbia University Press. 253 pages. $3.50.

A scholarly demonstration how this philosopher, critic, editor, and author of the "Dictionnaire historique et critique" influenced the English, although he knew no English and never visited England.

The Spirit of Voltaire. By Norman L. Torrey. New York, Columbia University Press. 314 pages. $3.00.

A careful study by a Guggenheim fellow who consulted a large amount of primary material in Voltaire's private library at Leningrad and in Paris. Voltaire's spirit is presented as a great civilizing force badly needed in periods of increasing religious and civil fanaticism, as at present.

The Problem of Historical Knowledge. By Maurice Mandelbaum. New York, Liveright Publishing Corp. 340 pages. $3.50.

An answer to relativism, as represented by Croce, Dilthey and Mannheim. The author believes that historical truth can be arrived at by attention to causal description. The ideal of objective historical knowledge is held to be consistent with the frequent necessity of rewriting history. Historical monism is denied even as a possibility of approaching something theoretically valid.

Logical Aspects of Educational Measurement. By B. Othanel Smith. New York, Columbia University Press. 182 pages. $2.50.

The book is a criticism of the precision of educational measurement.

It points out very distinctly where criteria for measure are frequently not met. This it does by a careful account of the meaning of any kind of measurement, the logical foundations of measurement and the requirements of instruments. Specific instances are discussed rather destructively, and, one must add, deservedly so.

Friedrich Paulsen, An Autobiography. Translated and Edited by Theodor Lorenz. With a Foreword by Nicholas Murray Butler. New York, Columbia University Press. 514 pages. $3.75.

Lychnos. Lardomshistoriska Samfundets Arsbok. Sweden, Almqvist & Wiksells Boktryckeri. 653 pages. 12 kr.

The 1938 volume of the annual of the Swedish History of Science Society. The contents include an interpretation of Descartes by Cassirer. There are about 200 pages of reviews, of which many are in German, French and English.

General Semantics. Papers from the First American Congress for General Semantics. Arrow Editions, 444 Madison Ave., New York $2.

Two dozen discussions from many fields.

The Book of Time. By Gerald Lynton Kaufman. 287 pages, New York, Julian Messner, Inc. $3.00.

A popular work, which, considering the subject, succeeds in a degree only and thus retains an interest for readers of this periodical. It is Kantian-inspired. It contains not only technical material on the measurement of time, but also an account of Ouspensky's dream of a three dimensional time. Even more dimensions are suggested.

Reality. By Paul Weiss. New Jersey, Princeton University Press. 314 pages. $3.50.

"Reality is the first system of philosophy in recent times to take individuals, instead of events, as the basic elements of the universe". The system is called Epochalism and is a variety of systematic metaphysics. The author shows a strong influence on the part of Whitehead and Peirce. The literary style is pleasing even in the midst of newly coined expressions and statements which, at least to this reader, are very perplexing. One must quote one such statement: "A supervening individual is a unity which has an intensity greater than that possessed by any of its parts—a condition which is fulfilled vacuously by any, irreducible, indivisible individual".

ANNOUNCEMENT

Fifth International Congress for the Unity of Science, Harvard University, September 5–10, 1939

PRELIMINARY ANNOUNCEMENT

The Fifth International Congress for the Unity of Science is to be held at Harvard University, September 5–10, 1939.

The theme of the Congress is *Logic of Science;* interest will center upon the relation of concepts, laws, and methods of the various sciences. Attention will be devoted to general problems connected with the unification of science, the relation of the physical and biological sciences, the logic of the physical sciences, and the relation of the biological and the socio-humanistic sciences. There will also be a number of special sessions and symposia concerned with special problems and fields.

The International Organizing Committee is composed of R. Carnap, P. Frank, J. Joergensen, C. W. Morris, O. Neurath, H. Reichenbach, L. Rougier, L. S. Stebbing. The American Organizing Committee contains E. T. Bell, P. W. Bridgman, R. Carnap, A. Church, M. Cohen, J. Dewey, H. Feigl, L. J. Henderson, C. J. Herrick, E. V. Huntington, H. S. Jennings, W. Köhler, S. K. Langer, C. H. Langford, K. S. Lashley, V. F. Lenzen, C. I. Lewis, R. S. Lillie, A. O. Lovejoy, R. M. MacIver, W. M. Malisoff, W. P. Montague, C. W. Morris, E. Nagel, F. S. C. Northrop, W. V. Quine, H. Reichenbach, E. Sapir, G. Sarton, H. Schultz, E. C. Tolman, W. Weaver, L. Wirth.

Professor P. W. Bridgman is the Chairman, and Dr. W. V. Quine is Secretary, of the Committee on Arrangements at Harvard University. The Congress is sponsored by the International Committee of the Congresses for the Unity of Science, the International Institute for the Unity of Science, and, in America, the American Association for the Advancement of Science, the Philosophy of Science Association, the Association for Symbolic Logic, and the American Philosophical Association.

A series of twenty monographs, entitled *Foundations of the Unity of Science* (and constituting the first two volumes of the *International Encyclopedia of Unified Science*) is now being issued by the University of Chicago Press, and helps to provide a background for the Congress. Three monographs have already appeared, and it is hoped that all twenty will be in print by the opening of the Congress.

Those requesting later notices of the Congress are asked to send their names and addresses to Professor Charles W. Morris, University of Chicago, Chicago, Illinois.